Reminiscences about I.E. TAMM

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I. E. TAMM

Under the editorship of E.L. Feinberg

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Recollections on the outstanding physicist, Nobel Prize winner Igor Evgenievich Tamm (1895—1971) have been written by his colleagues, students and friends. More than a leading theoretical physicist, Academician Tamm was a public figure in the life of his country and international relations of scientists, and bred a new generation of physicists. He was a lively person. This book shows the diversity of his personality — from the style and character of his research to purely human features in relations with students and associates in mountain climbs. The book is intended for a broad readership.

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TO THE READER

This book is about a scientist, a theoretical physicist of international fame, Nobel Prize winner Igor Evgenievich Tamm. It tells of how the man lived and worked in science, what his hobbies and ideological outlook were, how he coped with problem situations and got on with people in general. We think this a remarkable life-story.

You will read of a person with scientist friends the world over. Admired for his work, Tamm was also greatly liked. As Sir Rudolph Peierls writes, "The world would be a better place if there were more people like Igor Evgenievich."

Tamm's contribution to developing Soviet physics and his support of biological research is great indeed. The Russia of the last tsars, despite a largely illiterate population, saw big names in mathematics (father of non-Euclidean geometry Lobachevsky, and later Chebyshev, Markov and Kovalevskaya etc.), chemistry (Mendeleyev), physiology (Sechenov and Pavlov) and so on. But Russian physics was in the doldrums. People like Umov and Stoletov are remembered in our country but their names will hardly mean anything to you, the foreign reader.

Then came P. N. Lebedev at the turn of the century, the first man to measure light pressure. That apt experimenter established a first major school of physics but died in 1912 at the early age of 46. Russia had at that moment no important physics centres and few physicists, and all physics equipment had to be imported.

How then did the Soviet Union come to produce scientists who were only four years behind the USA in harnessing the atom and another eight years on launched the first artificial Earth satellite?

Igor Tamm's biography will give some idea of the lifestyle of the people who developed Soviet physics, men who first suffered serious difficulties in upholding true science against vulgarisers and false views, men always rich in spirit.

Certainly, Tamm was an exceptional personality and not typical of all Soviet physicists. Yet he emerged in a definite country at a specific time which this book will perhaps help you understand.

FROM THE EDITOR

This publication of recollections by people who knew Igor Evgenievich Tamm, the outstanding theoretical physicist, was initiated by the Theoretical Physics Department of the Lebedev Physical Institute of the USSR Academy of Sciences.[1] The Department was founded in 1934 by Tamm who remained its Head until his death in 1971. It now bears his name.

Important as it is to preserve the memory of those who have contributed much to the development of science, this was not our sole consideration in publishing this book. The part Tamm played in the advancement of science, in raising several generations of scientists, in the public life of this country and in promoting international cooperation among scientists lends a new meaning to Tamm's life. That meaning by far exceeds the personal contri-

[1] The Lebedev Physical Institute of the USSR Academy of Sciences emerged from a Physics Study set up in 1725 in St. Petersburg (as Leningrad was then known). The Institute as such was formed in 1934 when the Academy of Sciences moved to Moscow. Its founder was the outstanding experimental physicist in the field of optics S. I. Vavilov (1890—1951), who was the President of the USSR Academy from 1945 to his death.

The Institute owes much of its current broadness and renown to Vavilov who invited a large number of major Soviet scientists. These included L. I. Mandelstam (1879—1944) who is famous, among other things, for his contribution to the theory of oscillations, optics and radiophysics and developed a large following; I. E. Tamm; the opticist G. S. Landsberg (1890—1957); the radiophysicist N. D. Papalexi (1880—1947); the acoustician N. N. Andreev (1880—1970); and cosmic ray physicist D. V. Skobeltsin (b. 1892; from 1951 to 1972 Director of the Institute).

The Lebedev Institute is among the most major in the world. Discoveries made at it include Vavilov-Cherenkov radiation (for which P. A. Cherenkov, I. M. Frank and I. E. Tamm were awarded the Nobel Prize in 1958), the laser (for which N. G. Basov and A. M. Prokhorov became Nobel Prize winners), the principle of operation of modern accelerators (for which V. I. Veksler received the US Atom for Peace Prize in 1963), and the principles of thermonuclear fusion. The first 250 megaelectronvolt electron synchrotron was also created there.

N. G. Basov is the current director of the Institute which takes its name from the outstanding Russian physicist P. N. Lebedev (1866—1912) who was the first to measure light pressure (1899), thereby confirming the Maxwell Theory, and formed the first school of Russian physicists (S. I. Vavilov was his pupil).

bution he made to theoretical physics which gained him international acclaim reflected officially in a Nobel Prize and the title of Hero of Socialist Labour, his election to several Academies of Sciences, and so on. Above all, Tamm was a unique personality whose traits and range of activities deserve, in our view, the attention of those who did not have the good fortune to know him personally, above all the young who, though under quite different conditions and perhaps in quite different ways than Tamm, are deciding their lives.

Tamm linked us with the Einstein — Bohr epoch. For many he was a model both in science and in public life. He was a brave man by all standards, physical and moral; a scientist of a fine and powerful intellect; an unimposing teacher who taught by example and through friendly, benevolent criticism without "guidance" or tutelage; a true friend; a man at once bright and serious, charming and persevering, tactful and open-minded. He inspired affection and admiration in many and was himself generous in his friendship for others. Dynamic and indefatigable, Tamm was not one to drop a difficult line of research, any highly involved scientific problem, defence of truth or an attempt on a forbidding mountain summit.

We would like readers who never met Tamm to get to know all this through the reminiscences of his friends, his colleagues and his students. Is that achievable? Most of the contributors to the book are not professional writers. The reader may meet with some verbosity and repetitiveness in different authors. The editors have tried to play down those shortcomings while leaving the contributors liberty to express themselves as they wished. We trust the reader will be indulgent.

Some other difficulties had to be overcome in compiling the book. The greatest problem was that very few people were still living who knew Tamm during his brilliant ascent in science in the late 1920s and the early 1930s. Neither was it too easy to marshal the articles in their appropriate order. Finally we plumped for the simplest solution of putting them in alphabetical order of the surnames of their authors. We took the view that the book need not be read page by page from beginning to end. A reader usually suits himself as to the sequence in which he or she will read such a selection. To make such a decision easier, we have appended a list of contributors indicating their profession or personal relationship with Tamm.

We hope that the reader will get an idea, not only of Igor Tamm's significance as a man of science but of his personality, and will not consider the publication of the book inappropriate.

The editors would like to thank all who contributed photos.

E. L. Feinberg, V. L. Ginzburg

IGOR EVGENIEVICH TAMM
(A Brief Biographical Sketch)

Igor Evgenievich Tamm was born on the 8th of July, 1895 in Vladivostok. In 1898 his family moved to Elizavetgrad (Kirovograd now) in the Ukraine. There for many years his father was in charge of the construction of the local power station and tram lines.[1] On finishing the local high school, Igor Tamm studied for a year at Edinburgh University, but after the outbreak of World War I he was transferred to the Physics and Mathematics Faculty at Moscow University. Tamm's interest in public activities took him at times rather far from his studies. First he volunteered for the front as a medical orderly and later he became active in political life. He was elected a delegate from Elizavetgrad to the First Congress of Soviets.

Upon graduation in 1918, Tamm taught physics at the University of the Crimea at Simferopol (1919-1920) and then at the Odessa Polytechnical Institute (1921-1922). There he worked under L. I. Mandelstam, who was to have a profound influence upon Tamm's growth as a scientist and his subsequent research in physics. They remained close friends until Mandelstam's death in 1944.

From 1922 on, with only two short breaks, Tamm lived and did all his research in Moscow. For many years he headed the Theoretical Physics Department at Moscow State University. When the USSR Academy of Sciences was transferred to Moscow in 1934, Tamm was invited by S. I. Vavilov, Head of the Lebedev Physical Institute of the USSR Academy of Sciences, to organise its Theoretical Physics Department, which he headed till the

[1] Officially his post was listed as "Chief of the Water Supply and Electric Light Services" — *Ed.*

end of his life. Here Tamm did his most important scientific work.

Tamm investigated an extensive range of problems in theoretical and applied physics. He worked on classical electrodynamics and electron theory, the quantum theory of light-and-substance interactions, the quantum theory of solids, elementary particle theory, nuclear physics and thermonuclear fusion among other themes.

Tamm did his first research in physics (macroscopic electrodynamics) under Mandelstam in the early 1920s. To be more precise, he examined the electrodynamics of the anisotropic medium, and crystal optics in the theory of relativity. He followed that up with studies in Bohr's quantum theory and non-relativistic quantum mechanics, a new area, which had just made an appearance then.

Though Tamm began his work in science relatively late in life (his first research paper was published when he was 29), his scientific career was extremely vigorous and fruitful, especially in the thirties. In 1930 he published a very important paper which contained an extensive quantum theory of light scattering in crystals. Research into that problem had been prompted, among other things, by G. S. Landsberg and L. I. Mandelstam, both close friends of Tamm's and Moscow University colleagues, discovering the new phenomenon of combinational scattering of light in crystals. (An identical phenomenon in gases was simultaneously discovered by the Indian physicist Raman and it is often referred to as the Raman effect). The discovery was made by the two physicists on the basis of Mandelstam's profound theoretical ideas which, though clearly correct, were quasi-classical in substance. Tamm evolved a comprehensive quantum theory of the phenomenon. He quantized both the light and the elastic waves in solids, using a sound quantum concept (or phonons, as Y. I. Frenkel later called them). The general theory arrived at by Tamm revealed and explained both combinational light scattering in crystals and the Rayleigh dispersion phenomena. The quantum theory states that when combinational light scattering occurs, the photon either increases energy by absorbing the optical phonon — the quantum of energy of optical oscillation of the lattice (the optical branch) — or diminishes it by producing such a quantum in the lattice. The Mandelstam — Brillouin doublet in a Rayleigh crystal light scattering spectrum appears due to emission or absorption of a phonon of the "acoustic branch" (a similar phenomenon also takes place in liquids).

We can state that the very concept of "quasi-particles" in a multibody structure originated from such a systematic intro-

duction of quantized elastic oscillations (sound quanta) into the general quantum theory — a matter of primary importance.

In the same year 1930 Tamm published a series of papers on the relativistic quantum mechanics of the electron, just formulated by Dirac. This theory accounted for the spin properties of the electron and lead up to a natural explanation of the fine structure in the hydrogene atomic spectrum. However, some elements of Dirac's theory were so unconventional and sounded so paradoxical at the time as to be rejected by many physicists. This was especially true of the concept of negative energy levels in the spectrum (or positively charged holes where the electron background fills these levels). Therefore, the consequences of the theory which could be experimentally checked had to be thoroughly explored. This prompted Tamm to investigate light scattering by free electrons within Dirac's theory.

The correspondence method was used at the time to study specific quantum interactions of particles with the electromagnetic field. But Tamm chose the principles of quantum radiation theory. His cross-section formula coincided with a formula developed somewhat earlier by the correspondence method by Klein and Nishina. The importance of Tamm's work was by no means limited to a more rigorous calculations supporting the result already obtained.

First, this was the first ever consistent quantum electrodynamic calculation of a specific relativistic effect. In particular, it involved a very helpful procedure for calculations within the perturbation theory as applied to the Dirac particle case (Casimir later also used this method, which is known under his name).

Secondly, Tamm's research led to an elucidation of some fundamental points: he showed that scattering of even the "softest" low frequency light quanta by the free electrons in Dirac's theory is effected through intermediary states in which the energy of the electron is negative. Therefore, even the limiting case, that of the classical Thomson formula for long-wave light scattering by free electrons, cannot be obtained within the Dirac theory without allowances for negative energy states. This proved the futility of the numerous previous attempts made to exclude from theory the negative energy levels which were difficult to interpret in physical terms before the positron was discovered.

Over and above indicating the prime necessity of accounting for negative energy levels, Tamm, at the same time as Dirac and Oppenheimer, discovered that in the presence of an unfilled negative energy level the free electron must inevitably fall to the level interpreted in Dirac's theory as a positron. Having calculated the probability of the process, Tamm defined the life-

time of a "hole" (i. e. positron) under terrestrial conditions, i. e. in the presence of numerous electrons. The value proved quite small, producing a plausible explanation for the fact that the positron had until then not been detected experimentally (the particle was discovered two years later).

Around that time Tamm visited Holland, Germany and Britain. It was his first trip abroad as a fully-fledged scientist. He benefitted greatly from contact with such outstanding theoreticians as Ehrenfest and Bohr; from the friendly relations established with Dirac, and later on with Bohr; and from a short time at Cambridge working on refining magnetic monopole theory. His Western colleagues came to know Tamm and formed a high opinion of him. Ehrenfest singled him out as the most suitable successor to the Chair at Leiden University, the Chair founded by Lorentz and then held by Ehrenfest.

In the years that followed Tamm concentrated on one of the most attractive applied problems in quantum mechanics at the time, the quantum theory of metals. His research in that field has since become an integral part of the modern theory of metals.

The paper "On the Theory of Photo Effect on Metals" published in 1931 by Tamm in collaboration with his student S. P. Shubin, laid the foundations of the quantum mechanical theory of that phenomenon. The external photo effect — emission of electrons from the surface layer — and the volume light absorption in the internal parts of a metal were both explained by the theory. This theory was further developed and refined by other theorists.

Tamm's *Possible Binding of Electrons on Crystal Surfaces*, which was published in 1932, was his most important contribution to the theory of metals. In it Tamm discovered, theoretically, the existence of a special type of electron levels. Being on such a level, the electron is bound on the surface of the crystal, and can neither leave it outward nor enter the crystal. These "Tamm levels" were to play a very important part some 25 years later when the theory of surface and contact properties of solids went through a period of very vigorous development. This is especially true of the transition layer resistance theory which was extremely important for understanding transistor mechanics. However, Tamm gave up studying the quantum mechanics of solids. Nuclear force came to the fore in physics in those years and from 1932 to 1947 it became the focal point of his research.

But before taking up those matters we shall look briefly at one of the most noted works that Tamm did somewhat later (between 1937 and 1939): the theory of radiation he developed jointly with I. M. Frank for an electron moving at a velocity exceeding the phase velocity of light in a given medium. The

theory explained the physics and gave a complete quantitative description of the phenomenon discovered by Vavilov and Cherenkov at the Lebedev Physical Institute and named after them. One needed in those times wide, unbiased, inertia-free vision to guess that an electron moving uniformly and at a sufficiently high velocity in a medium could emit light. And all that seemingly contrary to what a high-school course in physics teaches — an electron moving uniformly in a vacuum emits no light. Discarding habitual thinking patterns, Tamm and Frank created a theory that launched a new line of research in theoretical physics. The subject studied was the interaction with radiation of fast particles moving in a medium (crystals, plasma etc.). Hundreds of theoretical and experimental investigations have been carried out in this area, and numerous new effects have been theoretically predicted. Quite a few of these works have found practical applications. The discovery of the effect and its theoretical explanation was an outstanding scientific achievement which won those who made it a USSR State Prize (1946) and a Nobel Prize (1958). However, Tamm chose as his main interest other branches of physics: nuclear force and elementary particle problems — a choice and switch-over very much characteristic of him. They became, with the exception of a few short intervals, his primary scientific pursuits for the rest of his life.

In this area his research into the action of beta-forces between nucleons (1934, 1936) is particularly noteworthy. Tamm started from Fermi's theory of beta-decay, and put forward the idea that nuclear force was created by exchanges of electron-neutrino pairs. The idea that field quantum exchanges could produce inter-particle forces was not novel. It had been suggested by Dirac and realised by him in conjunction with V. Fock and B. Podolsky as an explanation of the origin of Coulomb forces in photon exchanges. The idea, however, that pairs of particles, including particles that have mass, produce a field of forces was completely new. Moreover, and this is especially important, the idea was elaborated and refined in a somewhat involved and consistent quantitative theory that took account, among other things, of various possible tensor properties of the interaction operator.

In his very first report on the subject in 1934, Tamm produced his formula for the interaction potential operating among the nucleons. He proved that the interaction was quite weak compared with really existing nuclear forces. Consequently, though those beta-forces actually exist, it is not they that are responsible for the stability of nuclei. Before long Yukawa, on the basis of this work, was to show that nuclear forces could be the result of

particle exchange where the particles involved were substantially heavier than the electron. The existence of strongly interacting "nuclear" mesons was predicted theoretically and subsequently established experimentally. Tamm's study was the prototype and basis both for the meson theory of nuclear forces and for other similar researches that in their general features followed the structure of his beta-force theory. This discovery is among Tamm's most important accomplishments and he himself regarded it as his finest work.

This was not Tamm's only line in the nuclear force problem. At the same period he set to analysing existing experimental data on nuclear magnetic moments. The work he did with S. A. Altschuler led them to the conclusion (in line with Bacher and Schüler's experimental results) that the magnetic properties of the nuclei can only be understood given the assumption that the neutron possesses a magnetic moment. Moreover, Tamm was correct in his assessment of its sign and order of magnitude. The possession of a magnetic moment by a neutral particle sounded paradoxical at the time and Tamm's conclusions met with stiff opposition from many prominent theoreticians. Eventually, they were finally confirmed.

Hard attempts to clarify the nature of nuclear forces and the properties of the mesons were the salient feature of physics in the latter half of the 1930s. A theory that mesons were internucleon interaction agents with a spin equal to one gained currency. Tamm proved that such a particle did not have any stationary levels in a Coulomb field and this was something of immense importance for meson theory. Another problem cropped up at the same time: were there particles with higher spins? And if so, what were their properties? An attempt was made to develop a theory of a particle capable of various spin states. Tamm took the problem up with V. L. Ginzburg during the War; they finished work in 1945 and their paper was published in 1947. It presented the first-ever relativistically invariant wave equations for a particle with intrinsic degrees of freedom described in terms of continuous variables.

A review of Tamm's scientific work and interests in the pre-War years would be incomplete without mention of his interest in basic principles of physics. This led him to stand up for quantum mechanics and the theory of relativity, defending them against attack by hide-bound reactionaries (who were still quite influential at that time); it inspired him to work in popular science and take part in discussions of epistemological problems and other similar pursuits. It was this interest that led him to examine (with Mandelstam) the essence of the energy-time indeterminacy

relation in quantum mechanics. (The paper was published in 1945, after Mandelstam's death.) The study revealed that in a case of evolution of a quantum system with energy E indefinite to the extent of ΔE, the relationship $\Delta E \Delta t \gtrsim \hbar$ expresses itself in quite a definite way. Δt is the time during which the mathematical expectation of any dynamic variable not commutating with the Hamiltonian function undergoes substantial changes (by the order of its mean magnitude).

Tamm like the whole Physical Institute staff spent 1941 and 1942 in Kazan on the Volga. Those were hard times. While occupying himself as usual with the fundamental problems of physics, Tamm spent a lot of time and energy on problems vital to the war effort. He helped I. V. Kurchatov and A. P. Alexandrov develop anti-magnetic mine protection for the Navy (Tamm did the calculations on elaborate configuration magnetic fields).

After the War, Tamm began new research on nuclear forces. In his 1945 study he developed a new method of investigating particle interaction. The method was distinct from the perturbation theory techniques that were universally used in studying specific processes. The publication presented the "Tamm method", or the "Tamm-Dankov method" as it is often called since US theoretician Dankov also came up with it some five years later. It is based on expanding wave functions into series, not with respect to coupling constant, but to the number of virtual particles effectively involved in the process under consideration. This method found wide application where perturbation theory does not work because the interaction is not weak (nuclear forces) or resonance scattering takes place. Tamm's basic idea, in effect, was incorporated in a number of theoretical schemes, and, in particular, in a method which became popular about ten years later of taking into account some of the more "simple" Feynman diagrams (though the breaking off of a system of equations called for by the method cannot be justified from fundamental principles). It subsequently transpired that the method is a variation of the functional method suggested by V. A. Fock in 1934 which found no application at that time.

Tamm used the expansion-by-the-number-of-particles method in his studies into the stability of the deuteron. Later on, between 1953 and 1955, working with members of his staff he put it to use in a series of investigations related to photo production and scattering of pions by nucleons in the energy range in which nucleons are non-relativistic and the effect of nucleonic pairs can be disregarded (i. e. for laboratory energy of pions extending to several hundred megaelectron-volts). The semi-phenomenological theory thereby developed consistently availed of the possi-

bility of appearance, in intermediate states, of nucleonic resonances (with mechanical and isotopic spins equal to 3/2), treating this resonance on the same basis as nucleons. This was a bold initiative as most theoreticians in those days did not think it correct to view the resonance state, with its width of the order of a meson mass, as a full-fledged particle. However, as is common knowledge, baryon (as well as meson) resonances with their short life-time later acquired full particle status in physics.

The Tamm-Dankov method gained wide recognition in physics. It was further developed by Dyson, who put forward a relativistic (with regard to the nucleons) version (the "new Tamm-Dankov method"). The great difficulties that followed did not inhibit Heisenberg from applying this method in his unified theory of matter.

During his prolonged study of nuclear interaction Tamm took up other lines of research which, though they did belong to nuclear physics, were "macroscopic" and not directly related to nuclear force. Among such studies he conducted before the War, the cascade theory of cosmic ray showers (done in collaboration with S. Z. Belenky) deserves mention. Ionisation energy losses by cosmic-ray particles were given due consideration for the first time. Study of shower characteristics to the smallest energy values of the shower electrons and positrons became possible.

Tamm's post-War research in thermonuclear fusion and magnetic thermonuclear reactor theory can also be classified as "macroscopic" nuclear physics and had important technical applications. Tamm studied various aspects of the theory of high-temperature plasma in a strong magnetic field, and calculated the width of the shock wave front etc.

Subsequently, with the exception of the occasional piece of research work, Tamm became more and more absorbed in study of the fundamentals of elementary particle physics. In the early 1960s he focused his attention upon formulation of an elementary particle theory, including the elementary length problem. He tried to develop such a theory by modifying Snyder's idea on space-time quantisation. He took as his basis variable curvature momentum space. His ideas and first results were presented before the International High-Energy Physics Conference at Dubna in 1964 and the Conference on Elementary Particle Physics held at Kyoto in 1965. Research along these lines proved to be extremely demanding. With rare perseverance and resourcefulness Tamm carried on, exerting himself to the utmost even after being stricken in 1967 with the amiotrophic lateral sclerosis he was to die of four years later.

Serious as his condition was, with unflinching courage, great

dignity and clarity of mind, Tamm worked intensively until his strength fell drastically some six months before his death on April 12, 1971.

Tamm has a relatively small number of research papers to his name. The stupendous, never-ebbing amount of work that he put into the advancement of science is reflected in significance rather than in number.

We have given here only a brief outline of Tamm's contribution to science but the reader should be able to gain some idea of the qualities of Igor Evgenievich Tamm, the research worker. Firstly, Tamm always endeavoured to look at the problems of the day. As a rule, Tamm was not one to go on working for years on the lines of research he had initiated. He preferred to leave the development of his discoveries to others, no matter how promising further research of them seemed. Secondly, he possessed an excellent command of theoretical physics techniques, which were always at the service of the objective and never dominated it. His profound understanding of the physical essence of phenomena was supplemented by an ability to proceed, above all, from the properties particular to them. And, last but not least, he had an independent way of thinking, courage to advance new ideas and defend them.

This sketch cannot give the reader an all-embracing idea of the significance of Tamm's place in science for yet another reason: his activities were not limited to scientific investigations. Engaged in extensive research, Tamm devoted much attention to his duties as a teacher and a lecturer, as well as to practical and administrative problems.

As an Associate Professor from 1924 on and a Professor and Head of the Theoretical Physics Chair at Moscow State University between 1930 and 1941, he in concert with Mandelstam revised the structure and curriculum of theoretical physics studies in the Physics Faculty. This period saw him write *Fundamentals of the Theory of Electricity*, a textbook that went through several editions. When the Moscow Institute of Physics and Engineering was founded in 1945, Tamm organised its Theoretical Physics Chair and headed it for several years. Then for a few years he once again held a professorship at Moscow University.

Tamm was a brilliant and emotional lecturer who succeeded in infusing a new life into even traditional physics courses, a search for new truths and an awareness of modern ideas. Thereby he exercised a powerful influence upon students and young research workers. Carried away by his lectures, his students were attracted to scientific investigation. Many a student made his or her choice in life under the impact of Igor Tamm's lectures.

It has been mentioned above that Tamm's main scientific work was done at the Theoretical Physics Department of the Lebedev Physical Institute, a Department he founded and headed for so many years. From five or six research workers at the outset, the Department grew to several dozen top physicists. Almost all of them were produced by the Department. Many members of its original staff and younger research workers and postgraduates have become noted physicists and today work all over the Soviet Union. Some of them head research establishments. After Tamm's death, the Department was named after him.

The success of Tamm's educational work can largely be attributed to his personal manner of addressing students and younger colleagues. His guidance was never dictatorial, moralising or petty. He taught others largely by example and critical analysis. His example was one of amazing diligence and high standards of integrity in serving science; of objective assessment of achievements, errors and failures; and of respect for all his colleagues' views, be they scientists of world renown or undergraduates. His criticism was uncompromising. And still he would never hurt or insult the dignity of a young man when he criticised his work. His critical comments were direct, but always kindly where the mistakes arose in honest scientific ventures. Initiative and independent thinking were eagerly encouraged, but the standards Tamm set remained strict. The remarkable variety of scientific interests and even of research styles of his pupils is probably rooted in such relations with their mentor.

His integrity and high principles in opposing pseudo-scientific views were undoubtedly of great importance in his relationship with younger scientists. He never went in for rhetoric when he spoke in public, nor did he go for effect. Tamm was consistent in explaining the erroneous and pernicious nature of some false sensational doctrine that lacked scientific grounds, though, as sometimes happens, it had gained the support of a newspaper or some influential person. In challenging such views Igor Tamm would line up his arguments logically and convincingly. Any personal bias was totally out of the question.

As far back as the 1930s in his speeches and articles on the philosophical side of physics, Tamm took great pains to expound the essence of modern physics, calling upon younger philosophers to make a thorough study of the problems involved in the discussion. In no debate, no matter how heated, would he degrade himself by using caustic expressions. He would simply throw up his hands in astonishment when such language was addressed to him. In the late 1920s he got involved in a debate with hide-bound opponents as to the existence of electromagne-

tic lines of forces. Tamm did not confine himself to generalised statements, though he could simply have shrugged off the arguments of those ignoramuses. What he did was to present calculations for a specific magnetic field with magnetic lines densely filling a toroidal surface but capable of reduction to a single short closed line should there be the slightest of changes in the electric current. This demonstrated the conventionality of the "number of magnetic lines" concept. These calculations were included in Tamm's textbook *Fundamentals of the Theory of Electricity*. One might add that 30 to 40 years later they were to prove practical importance to the stellarator theory (one of the controlled thermonuclear fusion research lines), to which such toroidal magnetic surfaces are essential.

Tamm's decisive stand against pseudo-scientific doctrines in biology aroused wide acclaim. Having dedicated his life to physics, Tamm nevertheless believed that biology was destined to come to the fore in the next stage of progress in the natural sciences. He himself took to studying genetic code and encouraged biologists to come closer to physics to allow molecular biology to advance along truly scientific lines.

But there is more than just this to Tamm's public activities. He was a passionate believer in the Pugwash Movement, and made a great personal contribution to bringing scientists from various countries closer together. All this helped to build up his personal prestige which was already high and extended far beyond the limits of the international community of physicists. Tamm's personal charm cannot be broken down and explained bit by bit. It attracted many people to him, from first-year students to the great luminaries of modern science.

Tamm's contribution to Soviet science was acknowledged in the title of Hero of Socialist Labour; he was decorated with the Order of Lenin and the Order of the Red Banner of Labour; and awarded USSR State Prizes, the Lomonosov Gold Medal of the Academy of Sciences, and the Nobel Prize. In 1933 Tamm was elected Corresponding Member and in 1953 Full Member of the USSR Academy of Sciences. He was also a member of a number of Academies in foreign countries.

There were quite a few difficult periods in his life. Some were terrible events of the epoch, some personal misfortunes of which it is suffice to mention here the ordeal of his long illness which demanded exceptional courage and all his strength of character. He stood firm throughout all those trials without losing his joy in life nor his good will to others. The many honours that came his way in the last two decades of his life, tokens of respect from

colleagues and the public both in his native country and abroad did not change him.

Igor Tamm retained the full integrity of his unique personality to the end of his days.

S. A. Altschuler

TEACHER

I met Igor Tamm for the first time in 1933 when, becoming a postgraduate student of Kazan University, I came to Moscow to seek a supervisor for my research work. I approached Professor Tamm because his *Fundamentals of the Theory of Electricity* was a book very much after my own heart. It was written in 1928 and published a year later.[1]

[1] Its special role should be explained to the modern reader. Kasso, the Minister of Education and a reactionary in his views, introduced an "educational reform" in 1911. By way of protest a group of most prominent scientists resigned from Moscow University, P. N. Lebedev, the remarkable physicist, among them. As a result, academic standards deteriorated sharply. Tamm recalled that in 1916 he attended a course in the theory of electricity delivered, if he remembered rightly, by Professor Stankevich. Upon reaching the Maxwell Equation the Professor said: "That theory is so complicated that we shall leave it out of the course." Some of the University professors subscribed to the mechanical theory of ether as an entity basic to all electromagnetic phenomena. This drag on our physics stretched throughout the 1920s and even into the 1930s. The first to introduce a new line in physics was N. N. Andreev, who around 1917—1918 delivered an optional course in the theory of relativity. Prior to that he had published an excellent survey of the spectral theory of wave processes in optics etc. However, acting alone he could not bring about substantial change in the situation. The invitation of L. I. Mandelstam (in 1924 on the initiative of then the young professor S. I. Vavilov and of a body of young postgraduates and especially A. A. Andronov) and Tamm to join Moscow University's Physics Faculty was an excellent boost to academic standards, helping to raise them to modern requirements. Using many of Mandelstam's suggestions, Tamm gave his course for several years at Moscow State University and the Second State University (now the Lenin Teacher Training College). The course, profoundly physical and abreast of modern concepts and discoveries, was published in book form under the title mentioned above. The textbook revolutionised the training of physicists in this country. It might be added that the most interesting courses based on other

I was keyed up when I entered the office of the Head of the Chair of Theoretical Physics. A very vivacious man of medium height met me there. With rare tact he encouraged me to explain the purpose of my visit and after a brief interview agreed to be my supervisor. Tamm wanted to know what line I would like to take up — the theory of metals or nuclear theory. When I told him that it would be nuclear physics, his satisfaction was quite apparent.

The discovery of the neutron and the positron in 1932 provided a real boost to nuclear physics. From that time on the centre of Tamm's scientific pursuits shifted to nuclear and elementary particle physics. The objective he set me can be traced to a letter Tamm received from Dirac at the beginning of 1933. The British physicist informed Tamm that Blackett had apparently detected traces of a magnetic monopole, an elementary particle whose existence Dirac had predicted in 1931. Tamm hypothesised that the neutron was a combination of two magnetic monopoles with opposite charges. If the hypothesis was correct, the neutron would possess certain properties that could be checked by experiment. Tamm assigned me to investigate how the introduction of such neutrons into the nucleus would affect the superfine structure of atomic spectra. I had put a year's work into the project when Dirac's next letter reported to Tamm that Blackett had made a mistake: it seemed he had not found any traces of a monopole.

By way of consolation Tamm told me that each theoretician had a waste-paper basket full of wonderful ideas. Experimenters were luckier — their results were always interesting even if they were a foregone conclusion. In contrast, the inventions of a theoretician's mind were no good to anybody if they were not confirmed. But what I had done, as often is the case, was not wasted. The data I gathered about nuclear magnetic moments were to prove useful.

At the end of 1933 Stern and Frish produced quite a sensation with their measurements of the proton magnetic moment using the molecular beam method. The proton magnetic moment came out as almost three times the predicted value. As Landé showed, the nuclear magnetic moments known at that time (of nuclei

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principles by Y. I. Frenkel, a course of lectures by V. K. Fredericks, and translations from foreign languages were all published some five or six years later. But even today when we have the original course of L. D. Landau and E. M. Lifshits and other textbooks, Tamm's book still takes pride of place in our literature. It has been reprinted several times both here and abroad; the latest edition came out in 1976 — *Ed.*

I. E. Tamm
(in the early 1930s)

with an odd proton number) could be explained by the motion of a single proton, provided an anomalous magnetic moment was ascribed to it. Tamm suggested I try to explain the magnetic moment of nuclei with an odd neutron number by attributing

those moments to the neutron magnetic moment. In a fortnight it was clear that it could be done if one ascribed a negative magnetic moment, half the value of the nuclear magneton, to the neutron. The negative magnetic moment indicates that its direction is opposite to that of the spin of the neutron. The explanation of the presence of moments with both signs in the said type of nuclei was now quite easy to come by, and this was something very much to Tamm's liking. Our article on the neutron magnetic moment was recommended by Mandelstam for publication in the *Comptes Rendus (Doklady) of the USSR Academy of Sciences*.

Visiting Moscow in 1934, Bohr decidedly challenged the assumption that neutrons had other than zero magnetic moments. Tamm demurred but, despite long argument, failed to convince Bohr. Later it was shown beyond all doubt that a neutron does have a non-zero magnetic moment and that that magnetic moment was negative. The idea that the magnetic properties of nuclei with an odd number of neutrons could be traced to the neutron magnetic moment was also vindicated. Today solid bodies are being widely investigated using a method based on the presence of the neutron magnetic moment.

Tamm was a great scientist and he was a remarkable man. It was a piece of extraordinarily good luck for me to have a man of such calibre as my scientific supervisor and patron for so many years. He treated his students as equals. When he spoke to us he made a point of never using diminutives of our names regardless of our age. He was never petty or nagging in his attitude to us and he encouraged initiative and self-reliance in every possible way. During scientific discussions, however, he would not accept any compromise and was very exacting in his analysis of results, demanding the elimination of even the slightest flaw. He was extremely well-disposed towards his staff. Though his own original ideas and valuable advice were very often responsible for the success of his students' research, he usually stressed the importance of their work and never even mentioned his own part in it.

Take my own case, for example. In Kazan, when I was still working on the dissertation for my degree, I took up deuteron theory. Tamm drew my attention to the need to bring non-central forces into my calculations. Here is what he wrote to me on June 24, 1935: "Generally speaking, if I am not mistaken, in your theory the states where $l=0$ (that is those in which the term $\frac{l(l+1)}{r^2}$ is absent) apply in your theory only to the spin

compensation case. If that is correct, it follows that: either $l \neq 0$ and the state is not stable, or $l = 0$ and the spin is also 0 and the full mechanical deuteron moment equals zero. In as much as this version conflicts with experimental data, one might conclude that the basic assumption of the theory — the *main* binding forces are independent of the spin direction — is wrong. This conclusion, in my view, is of definite interest and merits publication." Six months later, on January 9, 1936, he wrote to me: "*Your* (Italics mine — *S. A.*) conclusion that nuclear (non-magnetic) interaction forces must depend on the spin (the last paragraph of your dissertation) is in accord with the results obtained by Fermi who proceeded from an absolutely different angle (*Physical Review*, September 15). Fermi's article is most interesting and account of the effect exercised by spin upon nuclear forces will, in my view, prove highly important for nuclear physics. It may well, among other things, bring new light to the entire nuclear moment problem because Landé's vector scheme, simple as it is, will not be applicable to compound nuclei."

While a very kind and tactful man, Tamm was adamant in opposing the admission of people who had no aptitude whatever for research work. It so happened that a publishing house editor joined a postgraduate course under Tamm. Busy with his editorial duties, he neglected his research programme. During the postgraduate examinations Tamm was extremely strict with the man and eventually insisted upon his expulsion.

Another case of the same kind. One of Tamm's former postgraduate students in later years allowed his scientific interests to slip. Once this fellow was on the official panel when a degree thesis was being defended. The man produced a very bad impression by supporting the clearly flawed degree thesis. Tamm reacted in the following way: "That's the way things are! One cannot serve god and mammon. In the old days crooks forged money to get rich, today they present theses!"

The public stands taken by Igor Tamm against certain pseudoscientific views on physics became widely known in this country. This is particularly true of his opposition to the mechanistic interpretation of electrodynamics and to attempts to discard or to distort the theory of relativity and quantum mechanics. In 1934, if my memory does not fail me, a discussion on the concept of force was staged at Moscow University's Physics Faculty. Tamm's contribution to it was listened to with great interest. He spoke most convincingly with great feeling and life. Circumstances were often such that only a man of great courage and high principles could venture upon such discourses.

Tamm's friends by way of a joke conferred upon him a cardboard award "For Hunting Down Die-Hards", and he was very proud of the award.

In 1939 I was sent to Moscow for six weeks to work at the Physical Institute of the USSR Academy of Sciences. By that time Tamm was already doing most of his work there. He saw me in his Miusskaya Square office. After discussing the main scientific problems, we moved onto the international climate. Those were alarming times with the War just round the corner. As our conversation drew to an end, I remember Tamm saying: "If only humanity were more rational and switched the enormous resources expended on preparing for war to solving the problem of how to use nuclear energy, the challenge would be met in a decade. Dozens of research establishments would be set up, factories built, physicists, engineers and technicians trained; and mankind would gain an inexhaustible source of energy."

Though Tamm devoted most of his time and energy to abstract theoretical problems, he was always convinced that genuine science would find a way to its practical application. I remember him saying once with great satisfaction that it looked like elementary particle physics had, after all, found its first practical uses: meson beams proved to be effective in treating malignant growths.

In 1941 Tamm and his family moved with the Physical Institute to Kazan. They lived there in a house overlooking the University courtyard and were my next-door neighbours. In the autumn of 1941 I joined the army. While on combat duty I received several cheerful and interesting letters from Tamm which unfortunately have not remained.

Shortly after his arrival in Kazan Tamm asked me to help him locate Nikolai Chebotaryov, a Kazan University professor, a prominent Soviet authority on algebra. I had known the man for a long time and was surprised to learn that he and Tamm had been at grammar school together in Elizavetgrad.

After my demob I was so starved for physics that I immediately set about calculating some deuteron properties. This line of research, which I had begun much earlier, proceeded from the assumption of the existence of non-central forces. In a few months I took a draft article to Tamm who was already back in Moscow. He read the piece in my presence without any manifest interest, then he took a 1940 copy of the *Physical Review* out and in no time found Bethe's article in it. All my results were there in black and white! He did not try to console me but advised me to give up investigations the pursuit of which was senseless in Kazan. Instead, in his view, I would do better to team up with

the local experimenters. The advice was easy to follow as even before the War my interest in nuclear moments had prompted me to keep in touch with the research E. K. Zavoisky was then doing. And though Tamm's own line of research was very remote from what we were doing in Kazan, he always maintained a vivid interest in our work.

Tamm firmly believed in scientific contacts and exchanges, so taking part in seminars, national and international conferences was always rather important to him. He had a good command of English, German and French which was a great help at such events. In 1934 at Moscow University Perrin, the French physicist and author of classical studies on the kinetic theory of matter, delivered a lecture which Tamm interpreted very adequately into Russian on the spot.

A knowledge of foreign languages was imperative for Tamm's students. Some time in 1933 he suggested that I should review, at a Chair seminar, a report on a new piece of research by Oliphant and Rutherford. My reply that I did not know English (in those days the physicists' lingua franka was German) was considered a lame excuse. I shall remember as long as I live what it took me to do that translation.

Teaching physics occupied much of Tamm's time and energies. In 1933 I was lucky enough to attend his course on quantum mechanics. The presentation was lucid, the substance easy to understand, and this not at the expense of quality of ideas. Tamm considered it inappropriate to follow a strictly logical scheme in presenting one's subject to students. What really mattered was the physical essence of the subject matter at hand and that the listeners grasped it. Once, with an air of profound satisfaction, he intimated that in his quantum electrodynamics course he had managed to put across to the students simply and clearly the theory of the anomalous magnetic moment of the electron presenting all the inferences and calculations in full. Tamm always felt that faculty members should be given an opportunity to engage in solid research work. He was indignant to hear some top officials of our higher education system say that faculty members should do research only in as far as it was necessary to teach their subjects.

Even in the last years of his life when he was struck down by illness and confined to bed, he carried on with his research work. And as was his custom, he would now and then joke about his passion for elementary particle theory. Once he said that making fun of him, Landau called him a fanatic, a workaholic who would stubbornly keep looking into the most forbidding

problems of elementary particle theory when he could doubtless have achieved much more in other fields of physics.

Igor Tamm was a great scientist and a very good person, a man who helped to shape the future of many a physicist in our country.

E. L. Andronikashvili

IN SCIENCE AND IN THE MOUNTAINS

Once I entered Artemy Alikhanyan's[1] Moscow laboratory and saw him discussing something enthusiastically with a beautiful young woman:

"Meet one of our staff-members. We are planning a small cosmic ray seminar in Yerevan. Igor Tamm, Arkady Migdal whom

[1] The names of Soviet physicists P. L. Kapitza, L. D. Landau, A. B. Migdal and A. I. Alikhanov occur frequently in the text. A. I. Alikhanyan was the director of the Institute of Physics in Yerevan (Armenia), and founded the cosmic ray station on the nearby Mount Aragats. V. I. Mamasakhlisov is a Georgian theorist. Physicist L. A. Artsimovich at the time described was Academician-Secretary of the Academy's Section of Physical and Mathematical Sciences and led the session on elections to the Academy.
To further assist the reader we provide the following notes.
P. L. Kapitza (1894—1984) worked from 1921 to 1934 with Rutherford in Cambridge (1930—34 as head of the Royal Society's Mond Laboratory). He and L. D. Landau (1908—68) were Nobel Prize winners and, in addition to being members of the Soviet Academy, were members of the Royal Society and other foreign academies.
Later the names of Kurchatov, Ioffe, Y. I. Frenkel, Fock and Leontovich crop up. I. V. Kurchatov (1903—60) was an experimental physicist who became a Member of the Academy. He led all the USSR research and technical work on atomic energy and thermonuclear fusion.
A. F. Ioffe (1880—1960), another experimental physicist, a Member of the Academy, founded the first major Soviet physics research centre, the Leningrad Physical-Technical Institute (which now bears his name) along with a number of subsidiary institutes. The Ioffe Institute has produced some of the greatest Soviet physicists who would refer to him among themselves as Father Ioffe.
Y. I. Frenkel (1894—1952) was a theoretical physicist and a Corresponding Member of the Academy, and a close friend of Igor Tamm.
V. A. Fock (1898—1974) and M. A. Leontovich (1903—1981, one of the Mandelstam school), were both theoretical physicists and Members of the Academy, and likewise among Tamm's friends. Leontovich worked for many years with Tamm at the Lebedev Institute and at other institutions.
References to them will be met in the text and footnotes — *Ed.*

you know of I'm sure, and a few others will be there. Won't you join us? We won't climb the Alagez, not in winter. But you've been there more than once, haven't you?"

"Do join us, what's stopping you?" the lady asked.

"Well," said I, "if I'm the first man to launch cosmic ray studies in Georgia, I don't have much choice."

"That's marvellous," they said in unison.

On the 1st of January, 1949 we boarded a first-class sleeper and the "seminar" began rolling on its own. Even in the train we gathered and talked science. Tamm and Migdal sat on the berth in the compartment, and two of us would make ourselves comfortable in the chair opposite. Artemy Alikhanyan would place the folding ladder sidewise and perch on it. Each would talk about his experiments or present some calculations. Finally my turn came. I was not very well up on cosmic rays and nuclear physics at that time, and my talking about liquid helium struck a discordant note. But Tamm was alight with excitement. Migdal, who had seen my experiments and was eager to make the results known to others, would put in a word now and then and kept firing Tamm's emotional reaction. I have used the phrases "alight with excitement" and "emotional reaction" deliberately. Tamm was a model of emotional perception in every aspect of science. He possessed an exceptionally keen perception of the aesthetics of scientific pursuits in addition to a truly immense communicative temperament. An unusually fast talker, he very often expected the other party to have a much better knowledge than he or she actually had. That somewhat inhibited my grasp of the baffling ideas he expounded. We spent three mornings, three afternoons and three evenings in this fashion before we reached Tbilisi. Cars were waiting to take us to Yerevan.

The evening after my immersion into cosmic ray physics and the theory of nuclear forces, the compartment "seminar" being over for the day, I began to sort out my impressions of Tamm. I had first met him in 1932, a year after graduation from the Leningrad University Physics and Mechanics Faculty, when I was doing research at the Central Aero- and Hydro-Dynamics Research Institute in Moscow. I passionately desired to become a theoretical physicist. That's how I came to attend Tamm's famous seminars at Moscow University. The young Tamm would now and then interrupt the speakers and explain to the audience the new and striking discoveries made in physics in those years. It was he who told us about the discovery of the positron, the properties of the neutron and what the newly discovered artificial radioactivity was all about. Talking on those subjects, he

conveyed to us the scope of the new phenomena and their huge potential. With very few exceptions these contributions were totally spontaneous. At long last I plucked up enough courage to ask Tamm to become my mentor in theoretical physics. He very kindly agreed. I still remember how he puffed at his cigarette, then took it out of his mouth, looked at the lighted end and suggested that I should read an article by the German scientist Hund on a spectroscopic problem, summarise Hund's work and come to see him a second time.

Enraptured, I took the copy of *Zeitschrift für Physik*. I must have read Hund's article over some thirty times and I came to realise that summarising it was out of the question for the simple reason that I did not really understand what Hund had done. I did not dare go back to Tamm. Many years later, when I grasped that physics was not entirely out of my reach, it dawned on me that such articles were Tamm's way of finding out who had the ability to take up theoretical physics, who had the intellect and courage to draw general conclusions.

The seminar in Yerevan lacked the spontaneity and hence the interest of the one in the train. Tamm read his paper, as did Migdal. Several items were reported by Alikhanyan. But as so often happens, the exchanges in the lobby were by far more interesting. When I say "in the lobby" I mean the discussions we had in the snow fields outside the town amidst the snow-capped mountain tops. Close to the ancient temple of Garni, a ruin since the 1679 earthquake, I stopped our jeep on the outskirts of a small village.

"Any wine, cheese and bread to sell?" I asked a collective farmer.

"Of course, as much as you want and more," he answered.

The purchase completed, we got out of the cars in a large glade and sat down in the snow.

"I have never used such a snow-white table-cloth," Tamm remarked, breaking off a hunk of bread.

"I've never acted as a toast-master at such an enormous table," I replied.

The southern winter sun was dazzling, and we could hardly see each other. A sense of closeness to nature took us. We felt a close-knit group, and only the violet dusk that came creeping in from somewhere over the mountains made us get up and get into the cars.

One evening we were Alikhanyan's guests. His Saryan landscapes and Galents still lifes were much admired by all present. The image of a pear in one of Galents' canvases has stayed with me. No matter where I travel, it remains in my mind's eye as

the standard of a magnificent painting. As to Saryan... Well, his impact on me has been overwhelming ever since as a nine-year old boy I visited the Tretyakov Gallery in Moscow for the first time.

Once, I remember, everybody gathered in my hotel room in the evening to indulge in small talk. I told them how my brother Iraklî had given up smoking and exuded nicotine for three weeks after the feat. Tamm wanted to make sure:

"Three weeks?" he asked.

"Yes, it was three whole weeks," I said adamantly.

"A very alluring story! I shall give up smoking immediately," and he stubbed his cigarette out on the ash-tray.

In a few months we met in the Great Auditorium of the Physics Faculty in University's old building. He threaded his way along his row of seats to join me.

"Elevter Luarsabovich! I've not smoked ever since that day! I want to report my findings to you," he said, speaking very fast as usual.

"Since which day?" I was nonplussed.

"You can't have forgotten, can you? That story of your brother giving up smoking? If that's so you'll have to give me a cigarette as a fine!"

Somebody offered him one and Tamm was soon inhaling with great pleasure.

"But you were right: I really did exude nicotine for three whole weeks!"

To tell you the truth, the evidence of such two witnesses notwithstanding, I still don't know how ex-smokers exude nicotine.

But let us return to Armenia, to where we had left off. The conference was over, the snow-white lobby left behind. We were about to set out for Tbilisi. Cars were waiting for us, and we were waiting for Alikhanyan. Alikhanyan was waiting for an important telephone call. Our departure for Tbilisi was post-poned hour after hour. We had gone out to the cars in the late morning, but when evening came, we still had not moved an inch. Totally fed up, tired and hungry we got out of the cars. Tamm was still arguing about the passage of a cosmic ray particle through solid matter, with Migdal as his opponent.

The next morning the cars were waiting for us again, and we were waiting for Alikhanyan and Alikhanyan was waiting for his call. But we had learnt our lesson. Nobody got into the car in the morning; we used Alikhanyan's office instead for our talking session.

"It's about time we left, friends." The inflection in Alik-

hanyan's voice implied that it was we who were responsible for the delay. We took our seats, and the motorcade set off.

A motorcade? Yes, indeed, that's what it was! With Alikhanyan ahead in the first car, accompanied by his Science Deputy and talking about the long-awaited telephone call. Tamm and Migdal were in the next car, then Mamasakhlisov and I. The rear was brought up by a van loaded up with spades, long sheepskin coats and other equipment. Posted on the running-board of the vehicle was Alikhanyan's Administrative Deputy, ready, it seemed, to fly to our aid should there be an accident or should we get stuck in a snow-drift. In this formation we reached the famous Lake Sevan and an eating-house on its bank, whose trout dishes were known all over the Caucasus. Migdal and Tamm decided to slide downhill on their feet. Migdal made it, but Tamm keeled over.

"It seems that a pair of skis is something one really needs here," said Tamm rubbing his bruises and brushing off the snow. "But one can do without." Bracing himself with a dose of optimism, he started up the hill for another try.

As we were not going to make it to Tbilisi, we turned to Abastumani on Kanobili Mountain. This was a detour, but the route took us to Evgeny K. Kharadze, the head of the Abastumani Astrophysical Observatory. Fate having landed us in Abastumani, I must mention that throughout the Caucasus no place can match it in tidiness and cleanness. No visitor, be he even a complete stranger to science, would dare drop a cigarette end there, or spit, or even speak too loud, for there are scientists at work almost everywhere. We spent a day with Kharadze, then we set out on our way to Tbilisi. There was, however, a stop-over en route: close to Borjomi a passionate desire to ski down Kohta Mountain seized Tamm and Migdal; so the two of them rushed off to catch a local mountain train and left us. We picked them up in a couple of days later. They were safe and sound and in fine mood:

"We've skied to our hearts' content! We did some ski-jumps, too!"

Back in Tbilisi we had supper at Mamasakhlisov's place. We were too tired to enjoy the wine, and the toasts were a bit half-hearted. But once we had downed what physicists call the critical mass, the mood of the party suddenly changed completely.

"Friends, we are on a macadamised stretch of the road now!" Tamm exclaimed.

Speaking out of turn and interrupting the toast-master, we all opened up and a free and vivid discussion began. We talk-

ed about the aesthetics of the research each of us was doing and of other physicists we knew such as Landau, Kapitza, Alikhanov... The party reminded me of the scene of free and uninhibited exchanges of views in Balzac's *Peau de Chagrin*, a novel that had made a deep impression on me in my youth. But when I re-read this book by my favourite French author I realised that the two scenes had nothing in common — our party with Tamm as the headman was so much more interesting and entertaining.

The next time I met Tamm I was already a full-fledged cosmic ray scientist. The younger staff members and we, the older generation, were constructing a small station on Elbrus 4,000 metres up to investigate the interaction of cosmic ray particles with the atomic nuclei of a target. Puffing heavily, a fully loaded 1.5 ton lorry would climb the steep slope of the icy mountain several times a day. A footpath had earlier been the only access to the place. When the "Eleven Mountaineers' Shelter" was being built the footpath was turned into a vehicle track. The lorry used it to clamber up the Elbrus. The engine suffered from oxygen deficiency, and kept wheezing all the way up, but still the lorry managed to negotiate the huge boulders as it struggled up the slope to higher ground. At times the lorry could have done with an ice-axe to help it get over an obstacle, especially at the last stretch of the track with its 20—25° gradient. Nowhere have I seen such steep slopes. Now and then the lorry would find itself facing a rockface. Without looking over his shoulder the driver would back her out, the pebbles tumbling down into the precipice from under her wheels. And so it would go several times a day: "up against a rockface" and "back out towards the abyss".

Once on our way out of Terskol I suddenly heard a familiar voice:

"My dear Andronikashvili! You'll give us a lift up to Icy Mountain, won't you?"

"Igor Evgenievich! Is that you? Of all places! You're welcome, I'll be delighted."

Tamm was not alone but with a party of several men and a woman. They all climbed into the back of the lorry. The flowers on the road overlooking Terskol grow so high that the woman could pluck them without even bending over the drop-side of the lorry. Then the hair-pin bends grew very sharp. Now and then some of our passengers would scream with fright.

"Will the lady, please, control herself," I said in a low voice without turning round from my perch on the running-board.

"It is not the lady," Tamm said bending over the drop-side. Tactful, as always, he dashed out in a barely audible tone:

"It's Professor X; he seems to be suffering from mountain sickness."

Tamm inspected our high-altitude station. While he admired the panoramic view of the Caucasian Range that opened out in all its splendour from the top of Icy Mountain, Tamm and I talked science, and he approved of my plans. Then he bid farewell and, bending under the weight of his heavy rucksack, walked off to catch up with his party who were struggling up the slope in Indian file. On their way back we met again. For a man of his age Tamm's agility in the mountains was remarkable. I told him I was impressed and surprised.

"Be fair, my dear friend. Elbrus! What sort of a mountain top is that, do you think? Nothing there for a mountain climber to show that he is worthy of the name!"

Later on we often met socially when I came to Moscow from Tbilisi and kept in touch with each other. In 1956 Tamm visited Tbilisi. At that time he was quite carried away by molecular biology and genetics. His primary interest lay in genetic code structure.

"Is it true that you are also in biophysics now?" he inquired.

"That's right," I said.

"And may I know what areas attract you?"

"I want to get at the difference between living and dead matter in terms of physics. On a molecular level..."

"Some job," Tamm replied. "And how are you going about it?"

"It means measuring the heat capacity of nucleic acids and proteins so as to cover the full range from absolute zero to denaturalisation level."

"Are you going to be able to cope with that?"

"Sure."

"When you do," he said with great feeling, "you must let me know. I'll come over to help you construct the theory of those phenomena."

"Thank you."

"Anything else of interest to you in biology? Does genetics interest you as well?"

"No, I have not given it much thought, I'm afraid. What interests me is cancer seen as a molecular phenomenon."

"Isn't that genetics too?"

"No, the way I see it it's thermodynamics."

"Very interesting indeed," Tamm repeated several times.

"And you think you'll be able to do it?"

"I hope so."

Whenever we met from then on, he never failed to inquire into my biophysical preoccupations. Progress was very slow, so I tried to avoid him. It took me 20 years to get ready the answers to the problems I listed to him way back in 1956. I stubbornly climbed my mountain all those years, doing it with zeal and dedication. Sometimes I felt that what I really needed was an ice-axe to help me get over the last obstacle, but the implement was missing and new round-about ways had to be sought.

I met Tamm for the last time in the summer of 1966 as he was dashing out of the Lebedev Physical Institute where elections to the USSR Academy of Sciences were being held. Noble indignation was written all over his face. I stopped him and asked:

"Igor Evgenievich, what's the hurry? And why are you so excited? Can I help by any chance?"

"I don't need any help, thank you! Artsimovich would not give me the floor when I was about to speak in favour of nominating Vitaly Ginzburg. It's just disgraceful!"

And he was gone, passing the check-point.

I entered the building of the Institute and got into the crowd at the election meeting. V. L. Ginzburg's nomination was never challenged and Artsimovich, who was in the chair, did not call Tamm just to save time. So the election of Ginzburg went off without a hitch.

N. A. Belousova

IN THE SUMMER OF 1920

We, the members of the "commune", lived then in "Sayani", an estate nine kilometres from Alushta on the Southern coast of the Crimea. We rented a summer cottage high up in the mountains with seven or eight rooms in it and a very large terrace all wrapped in wistaria in blossom. The plant formed an arch over the porch through which the sea could be seen far below us. My parents had invited a large group of friends and acquaintances to stay for the summer — eleven or twelve men and women all in all. Sometimes we would have overnight visitors, too. Each of us spent the day as he or she pleased, but we shared our meals, taking turns over the cooking. With laundry it was each man for himself. Food was not easy to

come by in those days, but plenty of fruit and vegetables made up for other shortages. The green stuff was sold to us by local Tatars, who delivered it to the cottage in baskets on their heads. Our boisterous, collective life was something of great interest to them. Only will I be able to recreate here the events of that merry and unusual summer?

I am trying to revive things that happened a very long time ago, and my account will be therefore somewhat fragmentary. Igor Tamm was a member of our commune. He was 25 years old then, a very dynamic young man of athletic build. I remember clearly how before long Igor became the life and soul of a not inconsiderable party which was diverse in occupation, age and inclinations, and unconventional. Naturally I cannot reproduce the long talks on scientific subjects that he had with my father[1], or the rather sharp exchange Igor had with his colleague and commune member Nikolai Ogloblin. A Professor of mathematics at the University of the Crimea, Ogloblin was a very dignified and morose person, rather pedantic and fastidious in his ways, whether it was washing up the dishes or washing his clothes. Tamm, on the contrary, was broad-minded, carefree, absent-minded, apt to forget things, dashing and impulsive, a very fast talker with a humorous touch in everything he said. His tact and consideration for others were disarming. When he failed to do some household chore he should have done, his apologies were forthcoming and sincere. He was easy to get on with, friendly and well-disposed to others. Igor's laugh was never subdued and always infectious. But sometimes his mood changed, he would withdraw into himself and avoid others. Igor missed his wife badly: she was on the other side of the front-line. He joined her later, risking his life in the attempt. But those spells passed, and Tamm would become his usual self again, helping out with everything the commune did by way of entertainment, a man easy to talk to and to confide to for all those around.

Kitchen chores were definitely not his cup of tea. He hated washing dishes, and frying-pans were his No.1 enemy. But washing up the kitchen utensils became almost traditionally a man's duty in the commune. The previous year the job had been done by Academician N. I. Andrusov, the geologist, whose family had shared a cottage with us. Hot water was in short supply and hard to come by, so sand and ashes (left-overs of the bonfires) were used for scouring the pots and pans. Such

[1] The biologist Professor A. G. Gurvich — *Ed.*

primitive mechanics went against the grain with Tamm, freedom-loving and enterprising man that he was. He preferred to act as water-carrier, to go shopping in the village; with a bagful of bread and vegetables over his shoulder he looked very cheerful and spry. Chopping wood was yet another duty he performed with vigour, although his enthusiasm sometimes endangered his toes and fingers, and he was a regular patient of my father's who would have to patch up his cuts and bruises now and then.

The young Tamm was very negligent about clothes. Of course, at that time none of us had more than very basic clothes. And what we had we saved mainly for the autumn and winter. The younger members of the party walked barefoot all summer *"ad pedes apostolorum"* as somebody rather loftily put it. A mime scene comes to mind here. The scene — our dining-room with our large company sitting round the table. Tamm was late for dinner. Then he appeared smiling shyly and hugging to his heart what was left of the clothes he was wearing. Sliding along the wall, he made his way past the long table and disappeared into an inner room. Soon it transpired that on his latest adventure he had wrecked his only shirt. The remains were barely enough to cover his chest. The long, hard attempt to mend the shirt was able to prolong its span by but a little.

Tamm was an accomplished swimmer. He would swim far out from the shore and stay there for a long time. Yet there was once he nearly drowned. He shouted to the boys on the beach to let them know that he was in trouble, but the noise of breakers covered his cries. His strength was waning. Tamm recalled later that he kept repeating to himself: "Don't waste your energy, man! It'll be more comfortable on the bottom." But Igor mustered up what energy he had and managed to swim back to safety. Once on a dark sultry night when you could not so much as see your hand in front of you, he talked the whole lot of us into following him to the beach to admire the fluorescence. Then he swam far out into the sea and with his body glowing all over, he cut capers in the waves to make the fluorescence still more striking.

Tamm took an active part in all the entertainment we arranged. Our hosts — they were a very large family — were all music lovers: playing the piano and especially singing were favourite pastimes for them. But music left Tamm cold. He much preferred painting. In the 1920s, after he had moved to Moscow, Tamm got together a group of friends and colleagues and they went to the Moscow Museum of Fine Arts on Sunday mornings to study West European art. When Dirac visited Moscow, Tamm

took him to the Tretyakov Gallery and then passed the duty onto me. Drama theatre (I remember us all rolling with laughter when we saw Kataev's comedy *Squaring the Circle*) and the cinema were for him a greater attraction. In the Crimea he would take part in charades with complete abandon. I forgot what the word was that we had to guess, but Igor was really magnificent as the Serpent in the "Temptation of Adam and Eve". Lying flat on a knotty olive branch and wriggling and smiling voluptuously he offered the shy Eve the forbidden fruit. The laughter was Homeric. His performance as one of "The Three Musketeers" was superb, too — his acting was free from any histrionics and quite true to character. Once on meeting us in the courtyard of Moscow University near our home, Tamm stopped to wave his make-believe plumaged hat and sweep it over the ground with such flourish that we expected to see a cloud of dust in the air.

Tamm's stay in the Crimea ended in the autumn of 1920 when he left to join his wife at Elizavetgrad.

E. S. Billig

THINGS I REMEMBER

I would like to tell you briefly about the wonderful friendship between the families of I. Tamm and L. Mandelstam.

From 1919 to 1920 I. Tamm worked with Y. Frenkel and A. Vagin at the Chair of Physics at the University of the Crimea at Simferopol. The University had at that time prominent scientists in many areas. Alexander Gurvich, the biologist and Leonid Isaakovich Mandelstam's uncle, ran a hard-working laboratory. Gurvich, Tamm and Frenkel forged an enduring friendship, and this despite a difference in age, considerable divergence of opinion on certain scientific matters and a not quite identical moral outlook. Perhaps it is to his contacts with Alexander Gurvich that Igor Tamm owes the "biological yeast" that fermented so strongly in him in later years.

Tamm's years in Odessa (1920—1922) I know only second-hand, from what my family had to tell me and letters that have survived. Odessa was going through grim times.[1] Gangs were

[1] The Civil War was still on.

L. I. Mandelstam

still on the loose, there was cold, famine and disease; food rations were lean and irregular, and food prices in the market-place constantly soared, with people buying in exchange for their possessions.

Igor Tamm came to Leonid Mandelstam with a letter of recommendation from Alexander Gurvich. As it transpired,

there was no need for it — Tamm and Mandelstam were quick to establish common ground and mutual understanding. From a formal standpoint, their scientific contacts were maintained through two channels. L. Mandelstam was in charge of the Chair of Physics at the Odessa Polytechnic which opened in 1919, an event he had contributed much to. On his initiative a laboratory of physics was set up and Odessa physicists such as Aganin, Tsomakion and, of course, Nikolai Papalexi[2] were invited. The same year a radioengineering plant was launched in Odessa. Mandelstam and Papalexi supervised tests at the plant's laboratory. The factory also produced the R-5 and other more powerful valves. Leonid Isaakovich at once drew Tamm into the work both at the department and the factory laboratory. There was a group of enthusiasts at the lab, lecturers and students who called themselves the "vacars" (vacuum group): Stakhorsky, Shchyogolev and Tamm set the tone. The three were eager, energetic and inseparable.

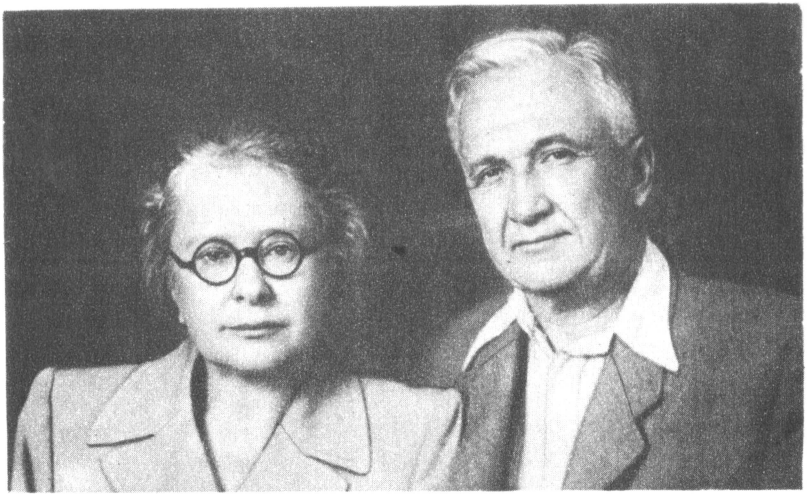
Tamm quickly made friends with the family of twelve with whom he and his wife lived. There were four children, three of whom were destined to become physicists. Their mother was Mandelstam's sister. Tamm was a special favourite with the children. They would not go to bed without hearing a chapter from the thriller *Percy Sommerville*. He made the story up as he went and pulled all kinds of horrible faces to make himself look like the characters.

This enthusiasm stayed with the children in later life. I remember, too, the heated croquet battles which occurred much later at Pravda Station where the Tamms, Mandelstams, Landsbergs and other families who were close friends all rented country summer cottages in 1947. Three generations took part in those matches and there was always a hubbub on the croquet field.

The Tamms were keen to make the friendly relations with the large clan of Mandelstams hereditary. Tamm saw himself more as a relation than a friend and neighbour. They were always taking out their children's families to get acquainted. Frequently greetings and congratulations bore the signature "The Tamms, old and young".

I must mention something that happened when the Tamms were still in Odessa. One day Tamm had an accident in his room. He poured kerosine out of a big tin into a bottle, and then from the bottle into the lighted lamp. It blazed up; the bottle fell

[2] Academician N. D. Papalexi (1880—1947) — radio-physicist and radio-engineer.



N. V. Tamm and I. E. Tamm (in the early 1950s)

on the floor and the tin exploded right under Mandelstam's feet. Luckily, the flames blew away from and not at him, and the fire was soon put out. I received a detailed description of the accident in a letter. Mandelstam was hurt most. He had to stay in bed for three weeks with very nasty burns. There was quite a lot of damage, and Tamm suffered an agony of remorse. To salve his conscience, the others, led by Mandelstam, discussed the accident merrily as a premeditated cold-blooded crime done by a crafty villain. Collectively, they wrote a poem about it, each ending in the refrain:

> Such people are not to be trusted,
> Beware of them whatever they may do.

From the early days of their acquaintance, Tamm saw Mandelstam as a man of great charm. This feeling grew with the years and at the end of his speech at Mandelstam's funeral he said: "I do not know for whom we are in greater mourning, the thinker or the man."

I first met Tamm in the early 1930s. I have many memories of him, but I will limit myself to just a few episodes. I remember a fleeting encounter in 1932, when Mandelstam and his wife — my sister Lidia — and I came upon the Tamms on a highway

in the Dombai.[3] The Tamms had a donkey with them to make the journey easier for Tamm's wife. We talked about Boris Gessen (then head of the Faculty of Physics) and his sick wife. Boris was a friend of Tamm's. That year the Gessens were also holidaying in the Dombai. Boris surrounded Mandelstam with attention and care. Was this the influence of Boris Gessen's schoolfriend Igor Tamm, or of Mandelstam's personality?

But the real friendship and personal contacts developed from our meetings in Borovoye[4] in 1942, the second autumn of the War. For Tamm his stay at Borovoye was a short respite after the hardships of Kazan. Conditions in the Academy's boarding-house, if modest, were an improvement on the War. Right after breakfast, Tamm would run from the big house to the little dacha where Mandelstam lived. They spent the whole morning working.

I had been evacuated to Borovoye after living out the first winter of the Leningrad Blockade. The recent past still filled my mind and when Tamm occasionally found time to wander with me down some of the many country lanes, I found it difficult to resist discussing tough experiences. For, eloquent man that he was, he was also a wonderful listener. More than a listener indeed. He always had helpful advice if asked.

Tamm's time in Borovoye coincided with the arrival of Alexei Krylov.[5] They and Mandelstam must have discussed the troubles of Moscow University's Physics Faculty. Two years later, speaking at a meeting to commemorate Mandelstam, Krylov was to say: "Mandelstam was honest and straight-forward by character, never sly or self-seeking. This earned him the respect of almost every professor at Moscow University. But in his last two years, the group of physicists at the core of the Faculty brought him much grief."[6]

The situation did not improve after Mandelstam went back to Moscow. While in evacuation, Leonid and Igor were expelled from the Faculty. The Theoretical Physics Chair, of which Tamm had been the head, was handed over to a young theorist, a former student of Tamm's who had no hesitation about taking over his teacher's job.

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[3] A Caucasus mountain holiday area.

[4] A health resort in Siberia where many aged and ill Academy members were evacuated in wartime.

[5] Academician A. Krylov (1863—1945), the mathematician and naval architect — *Ed.*

[6] *Academician A. Krylov.* Recollections and Essays. M., USSR Acad. Sci. Publishing House, 1956, p. 504 (in Russian).

Mandelstam and Krylov applied to the Committee for Higher Schools (CHS) which was in charge of all higher education establishments. Krylov and Mandelstam went to a meeting of the Committee, but the meeting did not take place. Mandelstam seldom went out for he was seriously ill at the time. The Committee's Deputy Chairman paid him a visit to discuss the situation in the University. Their conversation was long and frank, but nothing actually changed until much later, 1954.

Alexei Krylov was very anxious that Tamm be elected to the Academy of Sciences. There is a copy of a letter from him to P. L. Kapitza, sent from Borovoye on April 13, 1943, an obvious result of his talks with Mandelstam. Suggesting Tamm as a candidate for "the physics chair at the Academy", Krylov points to the significance of his work and in particular his study on the theory behind "the Cherenkov phenomenon". The letter ends: "If you think my reasoning correct, please add your voice to the nomination of Tamm."

On the day of Mandelstam's funeral, Nikolai Papalexi gave me his speech to read. The very idea of formal mourning seemed atrocious to me. I rushed to Tamm to persuade him that the ceremony should be held in silence. He probably missed the point of my confused explanations, and did not agree. When Tamm buried Leonid Mandelstam that day, he buried an old friend, a scientist and his teacher. Nikolai Papalexi, who was no less excited than I was, said a few heart-felt words. Tamm was very concerned to get Lidia's permission for the students to file past the coffin. He believed that the students should bury their teacher. He himself headed the procession.

I remember his frequent visits after Mandelstam's death to talk to Lidia on business and private matters, to sort out Mandelstam's papers. Mandelstam's death brought his students much closer to Nikolai Papalexi who until then had seemed remote and inaccessible. It was Papalexi who started to write a biography of Mandelstam for a collection of his works. He and Leonid had met at Strasbourg University in 1899. Mandelstam's death affected Papalexi very much.

In January 1947 Papalexi and Tamm both holidayed at the Uzkoe health resort in the suburb of Moscow. They spent many an evening at the chess board. On the evening of February 2, 1947 Tamm telephoned Lidia to tell her that Nikolai had been taken ill during the game. The second call was even more alarming and Lidia Mandelstam left for Uzkoe immediately. But by the time she got there, Papalexi was dead. Mandelstam's biography was completed by Landsberg, Tamm and Rytov.

* * *

In 1958 Tamm was awarded the Nobel Prize, an event which we celebrated in style at our place. Natalia Tamm was looking really smart. She wore an astrakhan coat which Igor had bought her in Stockholm. Mandelstam's widow Lidia, who always seemed to be nursing some sick friend or relation, received many rare medicines. Tamm gave us all a detailed and lively account of his meeting with the Swedish Royal Family, his talks with the other scientists and so on.

Tamm's visits often became "subject parties". Many must remember his stories about searching for the "abominable snowman", or his impressions of the journeys to the Kuril Islands, Kamchatka, China and so on. It was not so much what he talked about that was interesting as the enthusiastic way he talked. I particularly remember him talking on Gamov's paper about nucleic acid codes which had been published in *Nature* in 1954. It was a subject almost certainly quite new to him. Speaking animatedly about the significance and the principle of the code, and having acquired the basic terminology, he rattled off words like "adenine", "guanine", "uracil", "thymine" as if they were the names of friends. This intrusion of a physicist into biology, pushing the biologist aside and assigning him a secondary role, as it were, in deciphering life processes, invoked some protest from me as a biologist. But his enthusiasm was fascinating.

We had a very lively party to mark Tamm's completion of a series of works in 1953, his election to the Academy and his many awards. We presented him with models of a summer house, a car and "passes" to enter Mandelstam's house — all accompanied by suitable verses. On various occasions I wrote humorous verses to Igor Tamm, and they were invariably well received. I rather enjoyed being called a "Tamm's house poet."

L. A. Blumenfeld

A BIOPHYSICIST'S RECOLLECTIONS

It was many years my good fortune (from 1939 on) to be regularly associated with Tamm. I was one of the group of students who often gathered at the Tamms' before the War. Although many contributors to this collection can give a better, more complete description of Tamm the scientist and Tamm the man, I would like to relate a few incidents that have stuck in my mind.

After the War I went to the Karpov Physico-Chemical Institute as a postgraduate student, doing research on quantum mechanics. I followed the famous discussion between Einstein and Bohr on quantum mechanics in their papers published in the 1930s and 1940s, and Schrödinger's and Fock's works on the same subject. I got in a complete muddle and found myself agreeing with each of them. I just could not work out my own viewpoint. So I went to see Tamm. He said: "I am a convinced 'Copenhagenian'," and spent the whole evening explaining to me with much enthusiasm the validity of Bohr's concepts. He said that modern physics was moving farther and farther away from easily conceived visual images, from what seemed "common sense". He said this process was bound to continue, that the physics of tomorrow would become increasingly abstract and that its laws would contradict our experience more and more. No, not exactly contradict as fail to intersect this experience for existing in a different plane. Physicists were developing a new "common sense" of their own which would not coincide with our common knowledge. That was why it was becoming more and more difficult to write good popular science books on physics; soon it might be well nigh impossible. A sense of "understanding" could only develop from a long habit of abstract thinking within a system of mathematical and physical images which had no direct analogies in the world we saw around us. I do believe that for me this was the most important talk on science in my life.

Another episode that seems worth mentioning was to do with the well-known discussion on the resonance theory in chemistry, or rather with the very beginning of that discussion late in 1949 or early in 1950. The term "resonance theory" denoted the method Pauling proposed and Walland developed for qualitative analysis of molecular structure and the properties of chemical compounds. The technique was a quantum mechanics approximation method for a multi-electron system. This theory

has given a great boost to chemistry, especially organic chemistry. It in effect created the language chemists have been using for the last few decades.

Some physicists asserted that the resonance theory was idealistic (the basic argument in the debate) and unsound in that it contradicted the fundamentals of quantum mechanics. My teachers Y. K. Syrkin and M. E. Dyatkina, at whom the main attacks were levelled, suggested that I go to Tamm with them for his opinion. Interestingly, we had no hesitation as to which prominent scientist to go to. With his total scientific honesty, complete lack of "physicists' snobbery", and his inborn kindness and good-will, Igor Tamm was the only choice as referee.

Tamm was in a very good mood that day. During our talk he had a heavy baseball in his hands and every now and again he threw it to me or Yakov Syrkin. He knew nothing about the discussion or the resonance theory itself. It took him some ten minutes to understand the point and get into the picture. He said that the method proposed in the resonance theory contradicted nothing in quantum mechanics, that there was no idealism involved whatsoever and that, as far as he could see, there was nothing to argue about. Later everybody realised that he was right. However, the argument dragged on. There were people who called the resonance theory "pseudo-science", something which held back the development of structural chemistry.

I often heard Tamm lecture and never was he purely academic. The subjects he spoke on clearly interested him and he could always carry his listeners with him. I think that three of his lectures on completely unrelated subjects impressed me most of all.

Late in 1939 (or perhaps early in 1940), at the University Physics Lecture-Hall in Mokhovaya Street, Tamm lectured on nuclear force exchange theory. He said that in 1934 he had tried to base a theory on the light particle (electrons and neutrinos) exchange between protons and neutrons and had run into insuperable difficulties. In conclusion Tamm said that the Japanese scientist Yukawa had got over these problems by postulating (in 1935) the exchange of "semi-heavy" particles — mesons, rather than electrons (later these particles were actually found in cosmic rays). Nevertheless, Tamm's work clearly was of fundamental importance.

I also recollect his lecture on the A-bomb. At the end of 1945 or early in 1946 in the same lecture-hall he spoke on the two ways of creating nuclear explosives (based on U-235 and on plutonium). He treated these problems — which were new to most of his audience — with the utmost of clarity and precisions.

Tamm saw the full significance of the situation and tried to get over to a large audience that in the history of mankind the atom bomb had ushered in a new era and not just in warfare.

One event in 1956 has, in my view, proved of cardinal importance for Soviet biology. At one of the "kapichniks" (seminars at the Institute of Physics headed by P. L. Kapitza and held fortnightly for many years) two talks on genetics were presented. N. V. Timofeyev-Ressovsky gave a brilliant lecture on Mendel's basic concepts. The lecture mostly concerned the genetic effects produced by ionizing radiation, but a considerable part of it was a description of classical genetics. Tamm treated no less brilliantly the role of DNA in the transmission of hereditary information (the talk was based on the work of Crick and Watson and hereditary code theory). The assembly-hall was bulging at the seams, with the corridor outside and even the stairs packed. The importance of these talks can hardly be exaggerated. This was the first time in many years (since the notable session of the Academy of Agricultural Sciences in 1948)[1] that genetics was raised seriously at a scientific meeting. Tamm's lecture quite definitely attracted a new generation of young physicists and chemists to biology. This new blood was to give a major boost to molecular biology and biological physics in this country.

In the last years of his life, Tamm was bedridden. My visits to him, which always wound up in a game of chess, invariably began with his request: "Tell me what is new and interesting in your field." He was always happy to hear about new and unexpected developments. His eyes would sparkle, and he was never bored. He took an active interest in biology as in everything he had ever taken to. Tamm was always eager to help, to do everything in his power to promote a new area in science, to lend a hand in overcoming the inevitable obstacles of stagnation, inertness and indifference.

In 1958 a small group of third-year students from the Physics Faculty at Moscow University spontaneously decided to study biology. They began by inviting specialists of their own choice who gave them an optional course in biology. Then the

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[1] The All-Union V. I. Lenin Academy of Agricultural Sciences. At the 1948 session the influential leaders of this body (T. D. Lysenko and others) were scathingly critical of genetics. This unjustified, pseudo-scientific attack set back the biological sciences in the USSR. Things began to improve only about ten years later with the swinground in October 1964 when measures were taken nationwide to restore and develop genetics and other areas of biology — *Ed.*

I. E. Tamm and L. A. Blumenfeld at the Biophysics Chair, Moscow State University (1961)

students asked Tamm's assistance in introducing biophysical specialisation to the Physics Faculty. Tamm went there to talk with I. L. Petrovsky, Rector of the University, and managed to infect him with his own enthusiasm. The result was that the Moscow University Physics Faculty gained a new biophysics department, probably the first in the world to train physicists (as opposed to biologists) as biophysicists. If nowadays such departments are fairly common, it was Tamm who got the ball moving. For a number of years the development and indeed the very existence of our department was connected with Tamm. He was with us in thick and in thin. The mere fact that Tamm was around, a man of stature who shared our aspiration to teach biology to physicists, someone to whom you could turn for advice, gave a good deal of assurance and support in difficult times.

B. M. Bolotovsky

TAMM'S SCHOOL

I first met Tamm in 1946 in my second year at the Moscow University Physics Faculty. About a month before the new academic year, Tamm gave a public lecture on the path before the new Moscow Physics-Engineering Institute. Quite a few students from the Faculty came to hear him. We were all making up our minds whether to stay at the University or switch to the new institute. It is not so much the talk that I remember, as Tamm himself. He was a man of less than medium height, thickset, grey-haired and very active. He was never still, always pacing up and down in front of the blackboard, never looking at his audience. He gave the impression of having a plan of his talk in front of the eyes and reading from it all the time, for fear of getting in a muddle. He spoke with such rare clarity that we felt we understood everything. He spoke about the problems that still lay before theoretical physics and ways of dealing with them, and teaching methods at the new institute. The hall was packed and the audience listened raptly. When Tamm finished, questions just poured in. Tamm gave detailed answers even to questions that to me seemed stupid. And as he replied, he walked to and fro, as he had done throughout the talk, only looking at the questioner now and then. Afterwards he was surrounded by young people. I could not get near him. I could see over their heads that Tamm was talking animatedly, then listening attentively and then speaking again. His eyes sparkled, he smiled, became serious, smiled again. He had a very mobile face, as lively as the rest of him. My impression of Tamm after this meeting was that he was very good-willed. As I stood there looking, listening to his clear explanations, watching the attention with which he handled questions, his good-natured answers and his friendly attitude to the youngsters, I got to feel — and this was reaffirmed when I came to know him better — that he was a very kind person.

Another impression was that he had no sense of humour — he did not make a single joke during or after the talk. Later I was to find that Tamm had a sense of humour subtle and original, that he was always quick to see the funny side of things, and was an inveterate teller of funny stories some of which I shall remember to the end of my days. But this aspect of him was only noticed once you got to know him.

I did not go to the new Institute.

In third year we took electrodynamics and unfortunately we had two changes of lecturer in quick succession. Had any of the three lecturers run the whole course, everything would have been fine, but they were so unalike in personality, treatment of various aspects of electrodynamics, and even in manner that we came to the conclusion that there might well be three different theories of electricity. This impression would probably have remained with me, had I not bought the third edition of Tamm's *Fundamentals of the Theory of Electricity*. It and S. E. Khaikin's *Mechanics* largely determined my choice as a physicist. I now realise how lucky it was for me that Khaikin took the mechanics course in my first year. He used the same pedagogical principles as Tamm. Both were Mandelstam's students. Their prime concern was to reveal the physics of a phenomenon, to analyse the physical foundations of theory.

Tamm's book indicates a thorough knowledge of electrodynamics although he never mentions his own contribution to the field — which was considerable. The Vavilov-Cherenkov radiation theory (which I. E. Tamm worked out with I. M. Frank) is one of the most elegant works in modern theoretical physics. It is only very recently that Tamm's work on the electrodynamics of moving media has gained recognition. He first discussed that subject in 1924 in his first published work.

After *Fundamentals of the Theory of Electricity* was published, Tamm was often approached for advice in this field and he never turned anyone down. Sometimes very funny situations arose, of which he told us with a smile. Here are two of his stories.

One day two inventors came to see him. They said they could cut the price of power transmission because they had invented special wood impregnation that could transform wood into a very good conductor with a low resistance such as metals.

"This would have meant a revolution in power transmission," Tamm recalled. At this point he smiled slyly with a slightly mysterious expression.

"Well, impregnate a log and off you go and transmit power through logs. An unheard of economy for Russia with its wealth of forest. So I met the inventors. They were very secretive about their compound. I asked them whether they had tried their idea out. They said they had and that it worked. But they needed money to carry on with their work. I asked: 'What is your impregnation method based on?' They said: 'Give us the money and we'll explain.' I inquired: 'Can you show your specimens?'

"'Give us the money, we'll show you.'

"We talked at some length in this fashion. Finally, I said: "'I have the feeling that your invention is unsound.'

"To this they replied: 'Perhaps. But look, give us the money and we'll manage.'"

In the second story a certain physics lecturer came up with an idea for a new method of generating electricity. He went to Tamm to discuss it. His invention was this. A magnetised iron rod is placed inside a coil, a solenoid. Iron loses its magnetic properties at the Curie temperature, 758°C, so a coil with magnetised core is placed in a constant temperature chamber at 757°C, 1° lower than the Curie temperature. When the temperature is increased by 1° the core demagnetises and the coil produces an electromotive force of induction. Now we cool the system by 1°. The result is magnetisation and the electromotive force of induction. And so on. According to the author, the efficiency of the device should be very high, nearly more than one, for the expenditure of thermal energy is kept at a minimum with the temperature of the rod only raised a degree, and possibly even this might be reduced.

Tamm listened to the inventor and commented that the device would work of course, but that the efficiency was rather estimated. There is an expression in thermodynamics for evaluating the efficiency of a heat engine. In the ideal case it will be: $\eta = (T_1 - T_2)/T_1$, where T_1 is the absolute temperature of the heater and T_2 is the absolute temperature of the cooler. In the proposed generator, the temperature of the heater would be $758 + 273 = 1031$ K and the temperature of the cooler $757 + 273 = 1030$ K. Hence, the efficiency would never be higher than 0.001. The inventor did not agree and was most put out.

"And he made his engine and it worked," Tamm said. "Eventually, at one of the academic council's sessions this man took the floor to accuse me of setting up obstacles against progress in science and engineering on the grounds that I had opposed a new generator which, when built, had worked very well. I replied that my objections were not against the principle but on grounds of the low efficiency. Well, he wasn't bothered about the efficiency, he just went on saying that I was an enemy of progress."

"Do you remember it, Moisei Alexandrovich?" Tamm asked M. A. Markov who was there. "How wouldn't I, when I was the secretary at that meeting," Markov answered with a smile.

In 1950, on entering my final year, I began to attend regularly the Tuesday evening seminar of the Theoretical Physics Department in the old building of the Lebedev Physical Institute in

Miusskaya Square. Tamm was the organiser. However, from 1951 through 1953 he was engaged in some applied studies and appeared only rarely.

I was charmed with the atmosphere at the meetings. Nobody was afraid of asking questions and there were a lot of lively arguments with usually more than half of those present joining in. The older people, Tamm's students and close colleagues — S. Z. Belenky, V. L. Ginzburg, M. A. Markov and E. L. Feinberg — were well disposed towards their many younger colleagues such as G. F. Zharkov, Y. M. Lomsadze, V. P. Silin, S. I. Syrovatsky, M. L. Ter-Mikaelyan, V. Y. Fainberg, E. S. Fradkin, D. S. Chernavsky and many others who had just joined the Theoretical Physics Department.[1] The theorists from V. I. Veksler's laboratory, A. M. Baldin, V. M. Mikhailov, M. S. Rabinovich and Y. D. Usachev, were regular visitors. The old hands sat in the front, the young at the back. The young were noisy. Now and then someone would get up at the back and shout out an objection to some statement or assertion in the lecture ("young" and "old" read lectures equally often). These shouts from the benches were given full consideration. Here is a typical example.

The seminar is in full swing. The speaker, covered in chalk from head to foot, is working his way through the thick jungle of formulae he has written on the blackboard. Suddenly there is a shout from the back rows: Silin points an accusing finger at the blackboard. Silence falls. Markov, Ginzburg, Feinberg and Belenky look at one another, puzzled. They don't quite get Silin's point (neither do I). They try to work out what Silin is getting at. Feinberg is the first to break silence:

"As I see it, what Silin means is ..."

And he goes on with his explanation of what, in his view, Silin wanted to say. He speaks much longer than Silin did, but he makes himself much clearer. Listening to Feinberg, I begin to get the drift.

But Feinberg's interpretation draws objections from Markov who says:

"Zhenya, (short for Evgeny — *Ed.*), you've completely misun-

[1] These young research fellows and postgraduates were to become famous theoreticians and professors, some of them Members of the Academy. M. L. Ter-Mikaelyan organised and headed the institute in Yerevan, Y. M. Lomsadze headed a chair at Uzhgorod University, V. P. Silin and M. S. Rabinovich their own laboratories at the Lebedev Institute, and A. M. Baldin a laboratory at Dubna — *Ed.*

derstood Silin. What he meant was ..." says Markov, producing his own version. This version, too, is logical and clear. To me he seems to have got nearer to the point than Feinberg, and I am thinking: "Markov is right. He's expressed Silin's idea more accurately."

Then Belenky interprets Silin's objections quite differently from Feinberg or Markov. In his turn Belenky presents his explanation of what Silin has said. And I now see that this explanation is the best — he's really got to the heart of the matter.

Until Ginzburg shows that all of them are off the mark when he gives his interpretation.

It all gets very confused. Then it occurs to somebody to ask Silin himself (who had by no means taken all this lying down, but had been shouting interjections from the back). So now he gets the floor. And word by word, without a single change he repeats exactly what he said before. Again there is puzzled silence. And the whole thing repeats itself.

Finally, after a second or third round, the point is clear to all. It was really worth seeing the way the older colleagues looked at one another, how glad they were that they had understood and appreciated their opponent's point, even if he had expressed it not very well, and to answer him. In those years, when I witnessed such scenes, I saw mainly the funny side. Today what sticks out is the attention that the older colleagues paid to us, the young, the way they tried to grasp our questions so as to reply as fully as they could. "All are equal in the face of truth" — such was the law. No one was privileged in the discussions, though the argument might well be heated. The motto was to understand one's opponent's arguments, to grasp the nettle. In this way, our older comrades set us an example. I realise now that the unique atmosphere of the seminar was created by Tamm, even though he did not always attend the meetings.

He would come about once a month. Invariably smiling and cheerful, he opened the meeting as if it was he that was some poor relation from the provinces.

"Well, what's new in the world?" he would ask his closest colleagues. "We provincials are a bit out of touch. Enlighten us, please."

"As far as I can see there's nothing new. Nothing sensational at all," Ginzburg would reply.

"Did you read Heisenberg's paper? The one that appeared in the *Zeitschrift für Naturforschung*? On the *S*-matrix?" Tamm might ask.

Apparently some had heard about it but none had read it yet.

"Well, well, well. And I was hoping to get all the news from you."

And he started to tell us about it. The agenda was forgotten, the planned talk postponed. Everybody, including the unfortunate planned speaker, made themselves comfortable in their chairs in anticipation. Tamm spoke with the same clarity that had so impressed me at the first lecture at the Moscow Physics-Engineering Institute. He answered the innumerable questions in great detail. He had clearly read the paper and worked it through really thoroughly. If there was something that he had not quite understood or disagreed with, he went over it bit by bit showing why, what known data it contradicted and just where the paper did not fit the general picture.

"Does anybody see it more clearly than I do? Speak up."

There were always plenty of volunteers. But the problem was rarely simple and the explanation was often far from clear, as Tamm would quickly show. But if the answer was simple and convincing, he would be lavish with his praise. If he discovered any error of his own, Tamm would analyse his train of thought to show what had led him to the wrong conclusion. Of course, there was some self-flagellation in these analyses, but his main idea was to warn his listeners against the same mistake.

All present were invited to give their opinion on any problem. The characteristic feature of all the discussions was that both those who were right and those who weren't rejoiced at discovering the truth. In some miraculous way, the disagreements, that did emerge at times and which might well have led a newcomer to think he was in a labyrinth, were often the very things that helped to get at the right answer.

When discussing somebody's work, Tamm always substantiated any objections he might have. He never allowed himself the slightest disrespect for his opponent. A mistake is no reason for mockery. Speaking against a conclusion, Tamm not infrequently voiced his respect for the author.

In a 1950 seminar, Tamm was giving a talk on Heisenberg's paper on the non-hermitian Hamiltonian. The moment he began to speak, Vladimir Fainberg, sitting in a back row, shouted out some remarks. He was then 25. He was burning with youthful ardour (as he still is) and it was pointless trying to stop him. Fainberg declared that the non-hermitian Hamiltonian produces complex eigenvalues whereas a physical quantity is

incapable of having complex values.[2] Since Fainberg's remarks were not formulated as a question to the speaker, Tamm paid no attention and went on speaking. But Fainberg was not one to listen to something he did not like. He repeated at the top of his voice that, generally speaking, the non-hermitian Hamiltonian did not produce stable states, so what was there to discuss then? Again Tamm ignored him and carried on with his lecture, pacing up and down in front of the blackboard. Then Fainberg called out for the third time. Tamm broke off. Of course he had heard Fainberg's earlier interruptions and I even sensed that he had found them somewhat irritating, which might have been why he had ignored them. But this time he made a point of answering. Walking up and down in front of the blackboard, right-left, left-right, without looking at the audience, he said that Heisenberg was one of the greatest scientists of our time, that his achievements had enriched natural science, that he had made important contributions in many areas of physics. Therefore, his every idea should be treated with the greatest of respect and never be rejected out of hand. True, Heisenberg, like all of us, might be wrong, but the very mistakes of a scientist of such calibre were instructive and non-trivial. That was why Heisenberg's idea and analysis of all ensuing inferences should be treated impartially and very seriously.

This digression was spoken in a low voice and listened to in deadly silence. Fainberg did not interrupt again, and the young people were all quiet until the end of the seminar. But that was not the end of it. Soon after I witnessed the following scene outside the conference hall. Tamm reprimanded Belenky, Ginzburg and Feinberg.

"This is all your fault," he was saying to his former students, who stood there with bowed heads. "You spoil the youngsters. They have no respect for their elders." The three of them stood there silent and guiltily. I noted that his tone was sharper than when he had answered Fainberg at the seminar.

All good people have much in common. I remember Tamm's remark about his teacher Leonid Mandelstam. In his article "Niels Bohr and Modern Physics," Tamm wrote about the well-

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[2] V. Y. Fainberg remembers this episode somewhat differently, as seen from his recollections in this book. The difference is on the nature of Fainberg's remarks at the seminar. Having read my version, he said: "You may be right though I think it was causality, not hermiticity." Fainberg, probably, is more precise as to the physical aspect of the discussion. But what both of us are after here is the personality of Tamm, not physics. And in this respect our recollections coincide.

known discussion between Bohr and Einstein on quantum mechanics: "I would like to add that at that period Mandelstam was also very interested in Einstein's 'paradoxes.' No sooner did Einstein's paper appear with a new paradox in it than Mandelstam studied it thoroughly. And just a few days later explained the flaw in the reasoning in private conversation. I and other colleagues often asked him: 'Why don't you publish this?' He used to answer: 'Einstein is such a great man that all these considerations must be known to him. I may be overlooking something very essential in his reasoning.' Presently, Bohr published his reply and everything in it corresponded exactly to what Mandelstam had said. I do not write this to belittle Bohr, but rather to exalt Leonid Mandelstam."[3] This was not an idle reference by Tamm. He himself shared this trait with Mandelstam. But for all his esteem for Mandelstam, Igor Tamm was by temperament very different, he was a fighter. A born fighter. He could not remain silent when he disagreed.

Whenever he took part in disputes, Tamm always stuck to the central point and never demeaned himself by bickering or trying to discredit his opponent. Here is an example.

In the 1930s Tamm published a paper "On the Work of Marxist Philosophers in the Field of Physics" in the journal *Pod znamenem marxizma* (Under the Banner of Marxism). He argued that one must not discuss the philosophy and methodology of physics without first acquiring a basic knowledge of physics. He gave an example. A certain philosopher had suggested that the notion of force be excised from Newton's mechanics as being idealistic. He insisted that it be replaced by work done per distance unit. This author preferred the concept "work" to "force" as being more materialistic. Tamm objected to this by saying that in nature there are forces which do not perform any work at all, such as centrifugal force or the force that acts on a moving charge in a magnetic field. What about these? His conclusion was that to understand the philosophy of physics one must study physics as well as philosophy.[4]

On the following page, right after Tamm's paper, there was a paper titled "How Tamm Criticises Marxists." It was by the very man who wanted to eliminate force from mechanics. The very title showed just what the "offended" philosopher wanted the reader to think. The article stated outright that

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[3] Tamm I. E. *Collected Scientific Works*. Vol. 2, M., Nauka Publishers, 1975, p. 433 (in Russian).

[4] *Pod znamenem marksizma*, 1933, No. 2, pp. 220—231.

Tamm was criticising Marxists on idealist grounds. "In a land of Soviet government idealism cannot come out in the open, because it is foredoomed. So often idealism appears in disguise, masked even in Marxist-Leninist terminology ... In this context, Tamm's paper warrants the closest scrutiny."[5] These words could hardly be regarded as an answer to Tamm's point.

In this case, as in many others when Tamm spotted pseudo-science creeping in, when he witnessed unfair praise or blame, he stepped in boldly to restore justice and true science. This earned him great scientific and moral prestige, respect and popularity among physicists and in much wider circles.[6]

* * *

Upon graduating I was posted to V. I. Veksler's laboratory at the Lebedev Physical Institute. At the time he was engaged in developing new methods of particle acceleration, and therefore I had to study the classical theory of a charged particle propagation through a refractive medium. This theory was, in effect, given in the famous investigation of Tamm and Frank (1937) on Vavilov-Cherenkov radiation.[7] Later, in 1939, Tamm considered this problem in greater detail.[8]

The dragging of a charge by a moving medium (it had been studied by Tamm when he investigated Cherenkov radiation) was essential for understanding the mechanism of coherent acceleration proposed by Veksler who often referred to Tamm's analysis.

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[5] *Ibid.*, p. 233. This episode has now found comprehensive treatment in the works of Soviet philosophers and natural science historians. The six-volume *History of Philosophy*, in particular, makes special emphasis on the ideological significance of Tamm's work in 1934 (*History of Philosophy*. Vol. 6, part 2, M., 1965, p. 324) — *Ed.*

[6] It is no accident that the film director M. Romm asked Tamm to act as an adviser for the picture on atomic physics titled "Nine Days of a Single Year". Romm mentions this in his memoirs. (See Romm M. I. "Clarity of Vision". In: *The Screen, 1964*. M., Iskusstvo Publishers, 1965). In a letter to his colleague L. A. Indenbom, Romm wrote: "Tamm himself is the most amiable, old-fashioned intellectual, terrifically polite, lively and energetic. What a Sintsov (a nuclear scientist in a film — *Ed.*) he would have made! Tamm was delighted with the artistic side and that the physicists really looked like physicists. However, he was not quite happy about the number of those irradiated." (The letter is now kept by L. Indenbom's son, Dr. V. L. Indenbom / Phys.-Math /.).

[7] Tamm I. E., Frank I. M. *Doklady Akad. Nauk*, 1937, 14, p. 107.

[8] Tamm I. E. *Collected Scientific Works*, Vol. I, p. 77.

In 1953, after several years of research along lines outside the Institute's immediate interests, though they originated within the Department, Tamm gave himself over to the theory of elementary particles. That same year he was elected to the Academy of Sciences, and in 1954 he was honoured with the title Hero of Socialist Labour and won the State Prize. All this honour and respect did not change him a bit. Previously many of his friends had felt he was being unjustly passed over for awards, academic titles and prizes. But he himself had always been quite casual about it.

The years that followed Tamm's return to the Theoretical Physics Department of the Physical Institute were years of rapid growth in the Department in size and, more important, in quality.

The intervening years had seen remarkable achievements in quantum electrodynamics. That is why he began by studying everything that had gone before. Tamm organised a sort of ABC in quantum electrodynamics and general quantum field theory. A course of lectures was initiated in the Department which were given by the more advanced researchers. The objective was to reach world standards.

I am no expert in quantum electrodynamics so I can only give my impressions. Tamm listened very attentively, took copious notes, asked any number of questions and argued a lot. He must have been about sixty then but he was just as lively, interested and quick to react as any of the younger people.

Alongside this course, he embarked on studies on nuclear force theory. I remember how, at the end of a seminar, Tamm got up and said. "Comrades, there is a problem, volunteers are needed."

There was no shortage of volunteers. The original idea of the work and the initiative to get it started were usually Tamm's. Calculations were done in parallel by every member of the team, Tamm doing the most laborious and beating his younger colleagues to it, though he could have given everyone of them 30 years.

In 1955 I left Veksler's laboratory to join the Theoretical Physics Department. Nothing much changed for me though, for I had been there night and day since ever I was at the Institute.

Those were good years. Any researcher young or old could apporoach any colleague with any question, even Tamm himself, and wonders would happen. The person you asked for advice would put aside his own work to think over your problem. Then he would start telling you his view, quite possibly wrong, or even sometimes totally irrelevant. Long arguments flared up, sometimes lasting hours and days. Heated discussions raged at the

blackboard or in the corridor (some of the administrative staff at the Institute regarded the theorists as loafers because of such lengthy discussion in the corridors). Your head was in a muddle after these debates. Initial weak glimmers of comprehension died away under a heap of conflicting reasoning. But as the days passed you began to see that a great deal that you had heard was not that meaningless at all. Rather, it was you yourself who had missed what they had tried so determinedly to din into you. The advisers might not have got to the core of the problem, but they had given you a lead that could help you find your way out of the maze. This is nothing surprising. Nobody will give you an exhaustive answer to your problem, or rather an answer that will satisfy unless you sift it through. That is why we often make mistakes where other people have already made them. Of course, sometimes you could get a clear, precise and definite answer, especially if you approached Tamm or Ginzburg. But often even they could not escape discussions.

The atmosphere was conducive to expressing problems clearly, something at times very difficult to do. Even if an answer was not immediately forthcoming, the discussion always brought relief. At last it helped you formulate your problem (the psychoanalysts say that once someone is made understand what worries him, he will feel a sense of relief). Such discussions nearly always yielded fruit, though it might take some time to ripen. Everyone was prepared to give a boost to colleagues. This has a tremendous moral importance. A man has work more than enough, yet he puts everything aside for his comrade. That was the Mandelstam tradition, introduced to our Department by Tamm.

I was not interested in quantum field theory and was always silent at "field" seminars, and I took a great deal for granted that I now admire. My research was far from the "field" line which absorbed Tamm. I was well aware of his papers on the passage of charged particles through a refracting medium and I based some of my research on his results. But his earlier work no longer interested him. When he faced a new problem it took him over completely and he lost all interest in everything else. However, Tamm remembered my interests. One summer day in 1956 we met at the door of the Institute; he looked around mysteriously and said with an enigmatic smile:

"Come up to my study, I have something interesting for you," and shot off. He never walked upstairs, he ran. He did not go up two at a time, he sped from step to step as if running down the stairs and not up. His study was at the top of the building, right under the roof, four floors up. Tamm would

run the whole way. Very few could keep up with him without puffing and panting by the time they got there.

He showed me the Vavilov-Cherenkov radiation spectrum photographed by V. P. Zrelov in Dubna, a photo that was to become well-known. Two years later P. A. Cherenkov demonstrated it in his Nobel Prize lecture. It is very beautiful: concentrated coloured rings — the colours changing from red to dark violet as the radius increased — stand out against a black background. The photo is an excellent illustration from a physical point of view, because it shows the dependence of the radiation angle on light frequency. Tamm was absolutely delighted with it and expected me to be equally delighted. He looked from the photo to me and, smiling, said: "Superb, is it not?"

It took me a few minutes to catch what it was, but when I did I shared his delight.

"I shall take this photo to Geneva," he said.

The International Conference on Peaceful Uses of Atomic Energy was to begin in Geneva a few weeks later. Tamm took a very active part in it. Among other things, he gave a talk at a plenary session on Vavilov-Cherenkov radiation and showed Zrelov's photo. The talk was listened to with great interest. But Tamm's main concern was the nuclear force and quantum field theories. He spoke of results of studies and took part in the debate.

Describing his impressions of the Conference at the seminar, Tamm, as always, talked about all that interested him, and not just science. He recalled how he had learned water-skiing.

"We were taking a stroll by Lake Geneva, and I see a man sitting on a pier, dangling his feet in the water. He has a pair of water skis on his feet and a rope in his hands. The other end of the rope is tethered to a nearby motorboat. The rope is long and sags in the water. Then the boat gathers speed and moves away from the shore. The rope becomes taut, pulls the man off the pier and he slides over the water on his skis. I stood there for a while and looked on. Then I heard that I could take a ride too. I only had to pay. So far so good. I put on the skis, sat down on the edge of the pier, took hold of the rope. The boat moved off, the rope became taut and pulled me off the pier. The next instant I was floundering in the water, I couldn't keep my balance. I decided to have another go. At the second attempt I was gliding!" There was pride in his face as he spoke the last phrase. Later I heard the same story from an official with our delegation:

"Tamm was one of the most popular scientists at the conference. Everyone listened to him with respect. You really should act the responsible citizen when colleagues pay one so much

attention. So what does he do? He sits down on the shore of Lake Geneva, strips off his clothes and right in front of everybody he starts to learn water skiing, lands with a great splash and ends up floundering in the water."

About the same time, or a little earlier, Tamm began to take interest in genetics. This interest and his interest in biology in general was by no means casual. Boris Zavadovsky,[9] later to become a well-known biologist, was a friend from Tamm's schooldays. Zavadovsky's elder brother, Mikhail, an outstanding embriologist and endocrinologist, was at the same school four years earlier. After a year at Edinburgh, Tamm joined the Zavadovsky brothers at Moscow University. Boris Zavadovsky, an expert on Darwinian theory, got Tamm interested in modern biology. Later, between 1918 and 1920 Tamm came to know Soviet biologist A. G. Gurvich. They became lifelong friends and Tamm took great interest in his work. Tamm too was keen on the issue of life as a physicist. In his memoirs about Yakov Frenkel he wrote that in their years at Tavrichesky (now Simferopol) University they used to discuss the thermodynamics of life, on which Frenkel was then working.[10]

Since the late 1940s there had been virtually no research in classical genetics in our country. However, research in genetics was becoming utterly vital because, among other things, of the research on radioactive substances and other sources of radiation (the physics of accelerators, reactor technology and so on). We had to find out with all urgency what effect all these factors have on heredity. At Tamm's suggestion, I. V. Kurchatov organised a biology department at his Institute under V. Y. Gavrilov. A biology seminar was also set up at the Physical Institute, for which the credit should go to Tamm. The seminar dealt with many aspects of biology — the genetics of hereditary diseases, control of processes in living beings and so on. Tamm never missed a session. He had a firm interest in biology. He admired great discoveries, such as the DNA structure and deciphering the genetic code. He saw them as being just as important as taming nuclear energy. As always, Tamm got everyone around him interested. He wanted new developments in biology to become common knowledge. At his seminar he (and not only he) used to give very interesting talks on new developments in biology.

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[9] Member of the Academy of Agricultural Sciences, endocrinologist, founder of the Moscow Timiryazev Biology Museum — *Ed.*

[10] On the source of Tamm's long-standing interest in biology, beginning in 1920, see N. A. Belousova and V. Y. Frenkel's recollections in this book — *Ed.*

I remember my friends once asking me to invite a certain biologist to an interesting lecture by D. S. Chernavsky on auto-oscillations in biological systems. During the discussion, some of his conclusions were called into question and others more accurately defined. The most interesting comments came from M. M. Bongard. My guest listened carefully but didn't say a word, apart from asking at the beginning if Academician Tamm was present. All through the seminar my guest looked in Tamm's direction, the way people do at a celebrity. After the seminar, when I was seeing him off, he asked me if the lecturer had been a physicist or a biologist, and whether many physicists came to these seminars. Then he said:

"Tell me, has the lecturer established his priority on this subject?" I did not get what he meant and said so.

"Well, has he published yet?"

"Oh no, not yet," I said. "He was discussing his results for comments and objections first. It's only after that that he will write his paper."

"Surely, your colleague is taking a bit of a risk," said my guest. "Isn't he afraid to talk about it before he has submitted a paper for publication? Somebody else could get in first with it."

Of course this "get in first" meant steal, so I explained that this sort of thing never happened among us. The man listened with disbelief. Later I was told that at his Institute no one ever mentioned their theories or results before they had submitted a paper to a journal and received an acknowledgement from the editor, because there was always a risk that someone else would steal the credit.

Ethics in science was never discussed at the Theoretical Physics Department of the Institute. No one had ever spoken to us, advised or lectured us on such ethics — not Tamm or any of his disciples. But we saw them at work, and saw the way they respected each other's ideas and authorship. These were the living traditions of the Mandelstam school.

Here is a story that shows the man Mandelstam was. He once offered a problem to a postgraduate student. The young man spent some time over it, found what seemed to him a solution, and went to his chief. Mandelstam looked through the solution in his presence, checked the inferences from the formulae for certain limiting cases for which the answer was known. As the student's formulae did not fit in these cases, the solution clearly was wrong. The young scientist found Mandelstam's analysis extremely instructive. He went away, spent more time over the problem, arrived at another solution and went back to Mandel-

stam. Again analysis showed that the solution was incomplete and needed further work. This happened several times. Then finally, when he came back with his results, Mandelstam studied them in silence. Then he opened a drawer, took out a notebook, checked the student's formulae against his own and said: "That's fine. Now you can start writing your paper."

I'm avoiding chronological order because I find that every time I mention some feature of Tamm's character, other events come flooding back into my memory to illustrate that feature.

As head of the Theoretical Physics Department, Tamm never bothered much with administrative details. At meetings on such matters, he sat unusually silent, tired after the working day. But he was listening attentively and if something really important came up, he would readily give up any amount of time. I remember him often saying: "What must I do?" Where there was something he could do, he saw it as his duty.

Not infrequently these matters were not directly related to research. Take, for instance, housing: Tamm, wherever he could, intervened to help his researchers. That was in his nature. He also fought for promotion for us and often made provisions for members of the department to attend an international conference. I remember him dropping in on us and asking a young research worker why he had turned down an offer to go abroad to a conference. The answer was no money for the expensive journey. Tamm immediately said: "Go, I'll give you the money."

From 1958 I lived in the same apartment house as the Tamms, so we occasionally met at home as well. He was always considerate, amiable and friendly. But once he got rather cross with me. Having worked to 9 p. m. I was tired and, passing his door at the Institute, it occurred to me that if he was still there I could ask him for a lift home. I opened the door to his room. He was sitting at the desk writing. I asked him:

"Igor Evgenievich, are you going home?"

"What's that?"

"I just wanted to ask you for a lift."

Irritated at being disturbed, he growled without looking up:

"I'll go when I'm ready. Right now I'm busy, so don't bother me!"

I backed out muttering an apology though I could not see what I had done wrong. But on the way home I realised what it was that had irritated him. It was as if I had said, "Come on, stop working, I want to go home." I felt rather bad. But Tamm too must have considered that he had been a bit brusque. I learned

later that the next day, when I had already left for home, he had come to offer me a lift.

What sort of research supervisor was the man who founded the school of theoretical physics? I couldn't give a complete answer because I never actually was a student of his. But I do believe that he was a very kind, considerate and attentive mentor. I write "mentor" because he gave moral leadership and not only guidance in research. He was an example to his students in all matters of behaviour. Even now, years after his death, many of us who knew him ask ourselves what he would have done in a particular difficult situation.

Tamm never tried to make everyone fit a set pattern. On the contrary, he encouraged everybody to develop their individuality to the full. He rejoiced when younger colleagues came up with new ideas and was delighted at their successes. He was uncompromisingly critical, but not in a way that hurt our feelings or undermined our faith in our ability; he rather gave us the incentive to carry on and climb our mountains.

He recognized qualities in his colleagues that they themselves were not aware of. Often he seemed to have a higher opinion of his subordinates than that they had of themselves. In this way, he helped them gain the self-confidence so vital for a young research worker. He was a man of great erudition in the vanguard of science, and it is also highly important to the younger scientist to sit at the fount of knowledge. For all that, I believe the fact that so many well-known scientists consider themselves his students and disciples was as much for the qualities of the man as of the scientist.

I remember the spring of 1961 when Niels Bohr came to Moscow. At the end of April, some Moscow physicists gathered to meet him at Kapitza's Institute of Physical Problems. First, Bohr gave a talk on the complementarity principle, after which he answered questions. Among other things he was asked by Pyotr Kapitza:

"You have trained nearly all the famous physicists dealing with quantum mechanics and nuclear physics. What is the secret of your training?"

Bohr thought a moment, then he said: "I don't think there is a secret. Except that we were never afraid of making fools of ourselves in front of our students."

The interpreter was so shocked by this phrase that he could not believe his ears. He rendered it as:

"Bohr is never afraid to tell his student that he is a fool."

This provoked protests from around the hall. "No, he didn't

say that! That's not what he said." The interpreter consulted Bohr and corrected his mistake. Bohr went on:

"Another point I would like to make is this. However successful a student is in dealing with a problem, I always try to draw his attention to new problems that arise out of his results, to underline that research never really ends, that there is always much more to be done."

I doubt if Tamm ever even thought about what sort of supervisor he was. Nobody ever thought to ask him what his secret was in building up his school. Had he been asked, I am certain that he would have found it a difficult question to answer. I only mention this incident because Bohr's words reflected Tamm's position so well.

A great deal could be said, however, about Tamm's attitude towards his younger colleagues. Here is an example. A scientist in our Department was defending his doctoral thesis. He owed a lot to Tamm both in his immediate work and indeed for the opportunity to do research work at all. Tamm concluded his comments on the thesis by expressing a very high opinion of the scientist. He said:

"Over the last three years, I have been using the hypothesis of curvature of momentum space to quantize time and space. Throughout this work I have received a great deal of help from the author of the thesis, both in advice, very valuable advice at that, and in real actual guidelines. I would like to take this opportunity to thank him for his help."

Tamm's words, as well as being accurate, tell us much about Tamm himself. For though I have often listened to scientists defending their theses — and very good works they often were, defended by good physicists — I have never, before or since, heard such words.

Tamm's attitude to his own birthday is worth mentioning. His fiftieth birthday was celebrated at the seminar which happened to coincide with it. By chance, it was Tamm's turn to speak that day. Before the session, Evgeny Feinberg made a short speech and presented a chess set to Tamm. Accepting the present, Tamm remarked:

"You know, some tribes kill off their people when they reach fifty."

Evgeny Feinberg replied:

"I know of another custom: when a tribal chief reaches 50, he is driven up a tree, and then the whole tribe shake it. If he falls down, he is eaten. If he clings on, he remains chief. So now, Igor Evgenievich, please, show us just how firm your hold is on that tree."

And that was the whole birthday celebration, something which Tamm was very pleased at.

The chess set was an excellent present because Tamm always enjoyed the game. After work he liked to come to the room where younger members of the staff played five-minute lightning games, the loser making way for the next player. While Tamm waited his turn he followed the game attentively, delighted with good moves and sympathising when a player made some fatal mistake (sometimes the mistakes could be funny and he would laugh heartily with the rest of us). He was not a first-rate chess player but he enjoyed the game. At home he could get so absorbed in chess that he forgot everything and even took no notice when he was called to dinner. When a game ended he always said "Well, how about a return match?" regardless of who had won. Of course, he always played to win, but it was the game itself that interested him, attack, defence, counter-attack and getting out of trouble. He liked combinations. Having thought up an attack, he made his move and usually said: "Time to do something foolish." When he thought he had made a clever move he looked up from the board at his opponent's face. But if he made a bad move or some blunder, he would grasp his head and look desperate.

When Tamm was nearing sixty, the question of a birthday celebration arose again. He tried his best to prevent a grand meeting, and when he failed, he got out of Moscow for the day.

Another decade passed. We all wanted to mark his seventieth birthday. At first he flatly refused to attend a grand meeting. Then he reluctantly agreed, with the following reservation: "If there is a single serious speech, I'll be off."

The condition was accepted. There were a lot of humorous sketches, verses and songs. The party was very unusual and very gay. The Institute's conference hall (with some 600 seats) was crowded and everyone laughed heartily. Sitting at the front, the hero of the day was exceedingly glad and laughed more than anyone. The speakers and performers were Tamm's friends, his students, and his students' students. It was a pretty mixed gathering, but all were united in their love for Tamm and it was this that set the tone. At the end, Tamm got up on the stage and after prolonged applause he said:

"I have been much concerned with biology lately and I have discovered the law that controls the evolution of nerve brain cells. These cells are called neurons. And I have discovered that when a man reaches his seventies neurons break down to become 'foolons' — and he tapped himself on the forehead."

These words reflected his discontent at himself at the time. He was then working on the quantum field theory for a curved

momentum space. He said there were too many ups and downs in his study (a situation that never had been uncommon with him). His birthday fell in one of the difficult periods.

I remember visiting him in hospital, and his sitting on the bed in his pyjamas and telling me one of his unique stories. He was very fond of telling such tales and was an expert at it. He was as animated as ever though it was obviously taking it out of him a bit for he was breathing hard. At that point, an elderly lady came into the ward. Whether she was an acquaintance or a relation I do not know. I immediately rose to offer her the bedside chair. However Tamm jumped up off the bed, moving as fast as ever, and pushed a big heavy armchair across from the far corner of the room for his visitor. It exhausted him. It took him quite a while to get back his breath, but he sat with a contented smile.

A few years before his death, Tamm talked to a correspondent from the national press. He spoke about modern physics, the achievements and the problems. He ended with the comment: "I dream of living to see new theory and of being able to understand it."

Tamm was not the only one who believed that radically new theories were needed, that old concepts were no longer adequate. L. D. Landau expressed the same view. Both witnessed and took direct part in the scientific revolution which so tremendously expanded our understanding and knowledge with the arrival of quantum mechanics and relativity. The foundations of these two theories were fundamentally different from the pre-quantum, pre-relativity physics. Both quantum mechanics and the relativity theory had at first seemed illogical, non-sensical and utterly unclear. But as the years passed, relativity and quantum physics became established, habitual and self-evident. It had to be, it was inevitable; and of course we all realise that sooner or later these theories, too, may give way to new, radically different concepts, which allow for a still wider circle of phenomena. But when will the time be ripe for this new theory? Who knows? Tamm was one of those who worked selflessly to bring it nearer.

I sometimes think just how lucky I am to have known Igor Tamm. Moral instruction as to what people should do, what qualities they should have is not something one can learn from words. You need a living standard. I have had the great fortune to see several such people. Tamm was such a man. I hope I live to meet more men of his calibre.

I shall end my reminiscences with a situation in which Tamm appears just the way I remember him. It arose in 1959 or 1960. We were always short of space at the Theoretical Physics Department and normally three or four people had to share a small

room. So Tamm's study was never empty. In his absence V. I. Ritus and D. A. Kirzhnits used to work there. One day at lunchtime I looked in to call them to the canteen. A few minutes later the owner of the room came in. Greetings exchanged, the temporary occupants began to gather their work to move out.

"No, no, don't worry. Don't move, I beg you. I shall be off in a minute. I just want to have a look at my mail," Tamm said. "Don't take any notice of me."

But before he picked up his letters he asked:

"Well, what's new? Any new developments?"

Each one of us was always pleased if he could tell him something interesting. He took interest in everything — in scientific news, political events, anything in the Theoretical Physics Department or in the world. But it was not easy, indeed it could be well nigh impossible to tell him any news. He would listen politely and attentively to what you told him; then he would nod and begin to talk. He usually filled in several details and commented on what you had just told him. Soon it would become clear that it wasn't news to him at all; he had already thought it over and perhaps even discussed it with someone else. Well aware of this, we yet invariably told him anything we thought worth mentioning. We were on a safe base, for if he had already heard our news, he would add a lot of details and his own comments. And if it was really new to him, well that was wonderful, for there never was a better listener than Tamm, nor a collocutor more profound and witty. It was a pleasure to tell him one's story.

That day we had no news. We had no answer to his "What's new?" After a short pause Ritus said:

"Bolotovsky had taken up yoga."

"Really?" Tamm said. "And what have you achieved so far? Can you control your pulse?"

"Unfortunately I can't control anything yet. I've only just started," I said.

"Someone told me a story just a while ago," he said. So we got comfortable in our seats. Picture a room with a desk along one wall and a sofa along the wall opposite. Three men sitting on the sofa, listening eagerly, anxious not to miss a single word. Tamm tells his story pacing quickly up and down between table and sofa, from door to window and from window to door, walking in short, quick steps, head down, shoulders hunched, hands clasped behind his back. Occasionally a hand comes forward in gesture to underline his words. His face is animated as if he can see what he is telling. (And I believe he could, that in his mind's eye he saw every detail that he described). When he got to an exciting bit he quickened his pace, almost running as he talked.

On that particular day, the story was about a man who had been rushed to hospital with a heart attack. He was given several morphine injections to relieve the pain. A matter of days later he had a second heart attack, and was again given morphine. A few days after that, the patient walked into the hospital and asked the duty nurse for morphine as the pain had recurred. When the nurse refused, the man rushed at her threateningly, with his fists. "I'll kill you, you lousy skunk," he cried. So there was Tamm, fists in the air, running towards the door shouting: "You lousy skunk."

And at that moment the door opened and Vagradov, a theoretical physicist from Frank's laboratory, came in. He stopped short. For there was Academician Tamm, the Nobel Prize winner, rushing straight at him, head down like a bull about to gore, waving the fists in the air and yelling. For a second Vagradov stood transfixed. Then he said "Oh, sorry" beating a rapid retreat and swinging the door behind him.

By this time Tamm was at the door. He opened it and ran out. We had barely exchanged looks when he came running back, pulling the obeying Vagradov by the hand.

"Do excuse me, please," he said, "it really was not you that I was shouting at. I was just telling a story about a morphine addict who could achieve stenocardiac attacks at will. He got so good at it that he could produce an arterial spasm and was given morphia, which was exactly what he wanted."

Vagradov listened to his explanation seriously, nodding his head; but as soon as Tamm let his arm go he quietly eased his way to the door.

"It really has turned out rather awkward," said Tamm.

D. S. Chernavsky

ONE WEEK

Igor Tamm's lectures at the Moscow Physical-Engineering Institute were most unusual and energetic affairs. He was clear to a tee in explaining the crux. Every postulate was backed up by arguments and computations, and by his vivid gestures and facial expressions, by the very stoop of his figure. He reminded

me a bit of the actor Igor Ilyinsky in his best roles but there was no playing about it: Tamm never performed for the gallery, never posed. He hated poseurs. And yet he did have something of the artiste about him. Gesticulating was a part of him. On completing, say, his exposition of the fundamentals of the theory of relativity, he would thrust his hands into the pockets and incline to the audience. This was meant to emphasise that what he had just explained was quite simple and natural, and seemed difficult only at first glance. His whole body was an eloquent appeal not to waste a single minute but get down to this important and intriguing matter.

I knew Igor Evgenievich for twenty years, but I worked alongside him for only one week, in the summer of 1960. In that week I must have learned more about him than in all those twenty years.

At this stage I feel I ought to say a few words about physics. A scientist, his anxieties and preoccupations cannot be fathomed without an understanding, at least superficial, of the substance of his work.

Each of us has observed on some occasion a drop of water being condensed from vapour (or a crystal formed from an oversaturated solution). It appears as a result of minor random fluctuations. Where it will occur is hard to tell in advance. The whole process is the result of instability in the initial oversaturated state with respect to minor fluctuations, the embryos of a new phase. As simple as that it would seem. But from the viewpoint of "high science", i. e. theoretical physics, some curious paradoxes crop up. The initial state is spatially homogeneous, or, in physicists' vernacular, translationally invariant. An equation for this process is likewise translationally invariant. So a solution should be the same. Yet spontaneously appearing objects — a drop or a crystal — obviously do not possess this property. They upset spatial homogeneity: a spontaneous disturbance of translational invariance takes place.

Shortly before the events I am going to describe, Heisenberg suggested his variant of the non-linear field theory. It was intriguing to find out if special "drop-like" solutions, presumably corresponding to elementary particles, were possible. Tamm became interested in those special solutions of the non-linear field theory. The ultimate goal was to arrive at a solution corresponding to a microobject of finite energy and size. However, it was no easy matter to obtain such solutions or at least investigate them qualitatively. Tamm, as usual, invited all who wished to join the effort, and I was glad to accept. I was work-

ing then on modelling autooscillations in biological systems, and the phenomenon of instability and its role in physics were very close to me.

Furthermore, to solve the problem formulated by Tamm, the qualitative theory of differential equations and its methods, probably one of the most exciting areas of theoretical physics, would come in. The task had its neater points. The "phase portrait" was of the autooscillation type, but the limiting cycle looked so unusual that it even puzzled people engaged in autooscillation studies — the Andronova-Leontovitch group in Gorky.

It was a new and exciting job, at least ten years ahead of its time. Tamm's vigour and dynamism sometimes made me feel small. He never uttered words like "don't waste a minute", but that was what his example said. He rolled on "like a tank", crushing through all kinds of obstacles and technicalities. Soon we knew that the solutions we were searching for did exist, appearing in a completely homogeneous space spontaneously, namely as a drop of over-saturated steam. The objects had an odd inner structure, finite sizes and other interesting properties. The problem of translational invariance violation was solved in passing, with an ease and grace so characteristic of Tamm. Each of the solutions was not invariant of course in terms of quantum theory, but many solutions were possible — the centre of gravity of a "particle" could be at any point in space. Under the displacement transformation $(X \to X + A)$, each solution did not go over into its own self, but into a new solution equivalent to the former one. In this broader sense, suggested by Tamm, the translational invariance of the solutions was restored.

As the work drew to a close, a new paradox stared us in the face, and no solution is available to this day. A microobject (an elementary particle or quantum) appeared as a solution of equations in the classical (not secondary-quantised) non-linear field. Can one describe a quantum without secondary quantisation? That seemed a sacrilegious notion. Doubts were raised about the correctness of the objective set.

And then all of a sudden Tamm cooled — he could not work if the ultimate goal was not clear. The work was not finished (and not published, of course).

Several years later other theoreticians came to grips with autolocalised solutions of classical equations in field theory. Hundreds of works have been written on the subject. What was comprehended and learned in that one week is no longer new. Fifteen years have passed (These lines were written in 1975— *Ed.*). New brilliant results have been obtained. But the main problem, the one that stopped Tamm, is unresolved to this day.

Tamm is no longer among the living, and we know not what his attitude would be now. But he would hardly have betrayed his motto of being clear of purpose and full of endeavour.

D. S. Danin

QUANTA OF MEMORY[1]

Thousands of times recollections have begun with the words: "It was my great fortune to..." And each time it was the truth. For recollections are written of a person worthy of universal interest. That fate has favoured us by allowing us to meet such a person is reasonably seen as good fortune. So, banal or not, "fortunate" is the exact word. (Everything banal was originally accurate).

I was fortunate in seeing Igor Tamm for the first time in the company of Niels Bohr. No, no, chance by no means meant to be so mockingly gracious as to let a 20-year old student be the third at their private meeting. I was merely one of the lucky few who managed to make their way through to the lectures delivered by Bohr — first in the Great Physics Auditorium at Moscow University, then in the famous hall at the Polytechnical Museum. It happened in May 1934, during Bohr's first visit to the Soviet Union. To gather together everything that the memory has held onto about those greatest and yet a little too distant impressions is not difficult at all.

... The silence was penetrating, conservatoire-like and in it sounded meditative English speech. Bohr with his graying hair and slightly stooped back towered at the rostrum, half-smiling during the pauses. His voice was monotonously soft, yet it conveyed some unbending conviction. The whole of him was like that — softness and strength.

Foreign speech was exceptionally rare at the University and languages then were taught on an exceptionally low level. This left Bohr's for the most part young audience eagerly awaiting translation. The interpreter was to be Igor Tamm and that added to the audience's impatience, for "Professor of Physics

[1] An abridged version of the recollections D. S. Danin wrote for this volume. See *Nauka i zhizn*, 1977, No. 10 — *Ed.*

Tamm" even then ranked as an international celebrity. Besides, it was common knowledge that he was a brilliant interpreter: quick, at ease and witty.

... When Bohr's voice at length fell off, Tamm would suddenly take it up, as if protecting it from falling down, and swiftly recant everything in Russian. Bohr spoke about the first attempts to understand the structure of the atomic nucleus in the light of the recent discovery of the neutron. I cannot guarantee that the lecture included Tamm's theory of nuclear forces but what we, the audience, were witnessing often produced an impression of a dialogue between the lecturer and the interpreter. Speaking in a lofty manner with an impression of co-creation.

Their manners of motion and speech were directly opposite creating a sensation of a Southerner-Northerner dialogue (a purely psychological sensation that had nothing to do with their genealogy). Tamm, small, jerky and fast speaking, seemed always to be rushing so as not to get left behind while Bohr, rather tall and ponderously slow in delivery, let the words out into space not in flocks but in strings — now even, now broken, now calling some back to replace them with others. In such cases Tamm, who was making pencilled notes, instantly and unexpectedly in German asked him to repeat. Usually Bohr also answered him in German but then Tamm, having suddenly grasped things, was speaking English again, clearly challenging Bohr's argument. There then ensued a moment of merry confusion which everybody enjoyed because both were genuinely laughing... Tamm too was softness and strength.

My memory has retained one really typical gesture of Bohr's: the testing way he bowed his head, seemed to be seeking Tamm's agreement to what he had just stated; this made that sense of their being equal still greater, giving the impression that Tamm was co-author rather than interpreter of the lecture. Indeed, with the same gesture to the audience, Bohr seemed to be asking our permission to put his next argument. But there seemed to be something else to this: a call for understanding from all, from the front to the back of the hall.

Both halls I saw Bohr at were built up in the semicircular amphitheatre style and from time to time his eyes travelled upward to take in the whole arc from the bottom to the top row and seemed to get stuck somewhere near the roof. This made Tamm aim his look up there as well. And then everybody wanted to turn his head up to see what Bohr had seen, and Tamm.

Bohr's face then did not yet have that apostolic look which so enchanted in his later portraits (and incidentally in Tamm's

later portraits too). His face I remember more ordinary than it was later; and from a distance his full lips looked chapped. Nor did the rest of him bear any traces of that well-groomed European look which ill-dressed students, that we were in those lean years, might have expected from such a famous man. On the contrary, he appeared ordinary which frankly was hardly likely to catch the imagination of the young. I doubt if anything can be more discouraging in a celebrity; yet somehow precisely that can produce a great impression. Could it be that expectation of the distinguished looks of his portrait (like Newton with his shoulder-length curls) means that the real Bohr is remembered so well exactly for being outwardly so ordinary? Or, to put it another way, natural? Today we can definitely say that though he is no longer with us, his memory lives on.

I have written here what, after 35 years, I wanted to begin a book on Bohr with. But I would like to say that Tamm produced an equally big impression. He exuded the same winning feature of sincerity. That is just what he was like and how he is remembered; he too is no longer with us, and his memory likewise lives on.

For chemistry students (I had been one for three years before switching to physics) Tamm's scientific fame remained a mystery: we were not equal then to his theories, his contribution to quantum physics — the theory of light scattering in solids in 1930, or "Tamm's levels" in crystals in 1932, or the theory of exchange interaction within the nucleus involving electrons and neutrinos in 1934...

We used to repeat what we heard: that the still young Professor Tamm (he was in his late 30s) was an outstanding theoretical physicist with ideas all his own. Unable to appreciate either the physics or the mathematics in his works (the knowledge we needed to understand this "wasn't taught" at our department), we knew well enough that ideas of one's own were not necessarily expected and not frequently present even in professors' heads. So, when a jealous argument broke out in the University garden, or in the basement canteen under the Rector's Office, or at a party at someone's place as to whose department was best, the physics students' trump card was: "At our department lectures on quantum physics are given by Tamm himself." Nobody knew why, but everybody saw the point. This is probably because even our at times merciless university chatter never contained anything bad, sceptical or equivocal about Tamm. His name always remained pure!

The years and our increasingly sophisticated life made this more and more important. Perhaps that is why three decades later I made a slight mistake in a story about Tamm. It was a two-

minute piece for a film about Tamm produced by Marianna Tavrog. She asked me to start from the very beginning and I remembered the physical unit of "1 tamm", which was then in vogue among the physics students. I related that it was assumed that a certain human quality in no man's case could exceed 1.0 tamm (0.01 or 0.99 you were welcome to but 1.01 was absurd; exactly as for ß in the theory of relativity). Calling "1 tamm" with assurance a unit of decency I did not suspect then that I was mistaking a psychological construction for a testimony of the memory! In reality it was a unit of speed in speech...[2]

Vitaly Ginzburg noticed my mistake when the film was already on the screen. He was my senior by two or three years which gave me a loophole for saying: "That was your year's version, ours was different." But since two of my own fellow-students have also caught me out, there is nothing I can do except confess. The more so since my mistake gave the film its title of *One Tamm*. A unit of speed of speech would not have been worthy of this honour, a unit of decency was. Nevertheless, the strict documentary veracity of the film suffered.

Yet I am not sorry about what happened, because, even if accidentally, Tamm's moral elevation was thus documented in the shortest possible image. Documented through a slip of the tongue? Yes. In the long run, what we preserve within ourselves as our understanding of someone who is gone is also a document. And, psychologically, perhaps the most significant.

For all that, I have learnt my lesson: I am now much more careful about particular recollections of those fargone times... Though it would be appropriate for a former Physics Faculty student to tell something about Tamm as a lecturer, other than general impressions, to write the traditional: "Forty years have passed, yet his voice still rings clearly in my ears..." Alas, almost nothing now rings truly from so long ago. Only the general impressions are reliable.

There is one strange sensation I have not forgotten: his subject was a heavy job but he himself was light. He was always the same, trying to leave himself behind, talkative, improvising — be it in the solitude of a lecturer at the blackboard or surrounded by students on the stairs of the "physics wing" of the Applied Mathematics Department. He used to catch our questions in the air and instantly began to answer them with short pauses to drag on his cigarette which kept going out. He seemed to be ready for dialogue at any time but never for prophesy; that is why it

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[2] See S. V. Vonsovsky's recollections in this volume — *Ed.*

was so easy to ask him about anything one had not fully understood. It was also a pleasure to offer him a light: it gave you a feeling of useful dialogue even though in reality it was usually a monologue. Indeed, even his lectures felt like dialogues with the whole auditorium because (and this again turns the memory back to his duet with Bohr) he did not want "to just lecture on a subject" but in his own way longed for understanding and feedback.

His improvisations at lectures were also like that, creating an impression that it had just struck him how to explain something that was hard to understand. Other improvisations made one feel that his science was living history. We realised at such moments: history is happening... (The rarest quality of education is when it suddenly springs to life).

Certain true episodes could, of course, be reproduced here in the order in which they happened as illustrations, but they would never be in Tamm's words, for his exact words have been forgotten. After all, it hardly makes sense to call them "Tamm-like..." I might confess, however, that later, when many a time I was sorry that I had chosen literature and not physics, the most tormenting motive for such regrets were my recollections of the quantum physics world of ideas, temptations and perspective as represented by Tamm. It is indeed true that sciences, like music, are variously presented in lecture courses.

Twenty years later, in 1961, I told Tamm of these regrets: I brought him a respectfully autographed copy of my book on physics and physicists (science writing). It was entitled *The Inevitability of a Strange World.* He cordially and quickly pronounced "Oh, thanks a lot", and quickly leafed through it, smiling at his photo on one of the pages — "It seems I was once young, too." He also noted with interest, "So it's true, no formulas" and continued:

"... as to your regrets, I think you'll agree that it is more important for you to hope that literature won't regret that you chose it instead of physics!" He laughed over the whimsical elegance of the phrase and then added, embarrassed: "No, no, I didn't mean it that way. I only wanted to wish you success..."

I have often repeated these friendly yet warning words to myself. Someone writing recollections cannot escape himself, hard as he may try; were it possible the recollections would become impersonal and meaningless.

Tamm said "So it's true, no formulas" because he had known about the idea of the book.

Two years earlier, in the summer of 1959, an International Conference on High Energy Physics (the latest Rochester Con-

ference) was held in Kiev. I had the luck to get permission to be present as an observer (reporters were not admitted there and nothing could have suited me better as a writer than the opportunity to observe).

Not that the conference went off without some funny happenings. For example, one night at dinner in the restaurant of the Ukraine Hotel, where the participants were staying, I found myself sitting next to Tamm. While awaiting their coffee the great Academics were exchanging tricky questions and joking answers. Tamm wrote the numbers from 1 to 9 leaving out the five on a paper napkin and merrily asked us what it meant. Two or three voices immediately replied with "No appetite!"[3] He laughed:

"Of course, quite true 'the five is missing!' I knew that everybody knew it; but here is another task and it takes a really good mathematician to solve it..."— and he carefully drew two rows of numbers:

1, 4, 7,
11, 14, 17...

"You are to find the principle of this sequence and write down the next number — the seventh member in the series, so to speak. Well, who wants to begin?" And he passed the napkin on.

Tamm went round the table with growing joy rejecting version after version: "No, not that", "Fascinating but not right." Then either A. I. Alikhanyan or A. B. Migdal said it was an excellent chance to "play a trick" on Landau who was at a nearby table. He came up readily, bent over the napkin and instantly replied: "The next can't be 111 — otherwise it wouldn't be funny!"

"Dau (Landau's nickname — *Ed.*) is right but he is still evading the question," parried Tamm, and his delight made him young. "We were asked this in the first form at Elizavetgrad school! The point is that we could only draw straight lines then. So it is easy to see that the next number is 41..."

He drew a large 4 and 1 in straight thick lines and passed the napkin on round the table again. This quite elderly man was seen reliving a happy childhood and youth. I saw him in much the same euphoria again at the chess-board in Gargy and Moscow.

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[3] Play on words. In Russian, «Аппетита нет» (no appetite) sounds almost like «А пяти-то нет» (But the five is missing) — *Ed.*

It was that night in Kiev that I got telling Tamm of the book which I was then writing about the "inevitability of a strange world" in nature. The event stuck firmly in my memory because of a remark of his that rather embarrassed me. In Kiev the summer heat lingered late into the night, so Tamm suggested after dinner that we all should "jog down to the Dnieper." His collegues had already made arrangements for the evening, so I was the only one that went.

We had hardly started down towards the Kreshchatik when he said it had long been his wish to hear "something truly interesting about literary life." I was still humming and hawing trying to remember something interesting, when he asked me as if in passing: "And what are you working at just now?"

I replied exactly like a person who in all earnestness answers the question "How are you?" (the classical definition of a bore). Tamm listened, patient. My plan to interest non-physicists in a physical "drama of ideas" met with alacritous approval from him but he immediately disagreed with my opening: it began with the dramatic story of the Aragats Mountain Laboratory's failure to discover varitrons in the 1940s ... I cannot reproduce everything he told me then but the essence was as follows:

"... Scientific research welcomes dramas only when there are results, no matter whether positive or negative, but a true result! Otherwise people have a right to suspect the drama may not be scientifically grounded. Don't you agree?"

My sole defence was my inner conviction that the "varitron drama" was well grounded. But much to my annoyance, our conversation was interrupted: under the picturesque Kreshchatic lights Tamm was recognised and instantly drawn into some academic polemics by two foreign physicists in sturdy base boots. Both the subject and their fast English being beyond my grasp, I left with a "see you tomorrow", despirited because of Tamm's disapproval. I think he was aware of it because when we ran into each other in the hotel lobby next morning he said, quite unexpectedly, as if our conversation had not been interrupted at all: "... and without formulas — it will be very good if you can manage that." This I understood to be a requirement of his magnanimity: if you cannot honestly approve but can honestly give encouragement, you must do so by all means; you must certainly not forget to do so!

Memory radiates quanta recollections, and randomly at that. A short parable about memoirists has come to mind. Tamm would have enjoyed it ... In May 1967, when the First Conference on the History of Nuclear Physics opened in Brooklin, Massachusetts, Victor Weisskopf frankly confessed:

"Once at school the history teacher became most annoyed with me: 'You don't know any dates.' I replied: 'Of course I know my dates — I know all of them; I just don't know what happened on which.'"

Everybody laughed and they had been laughing at themselves as well: events and dates were usually divorced in their memories too. They disagreed with each other even on such dates as the discovery of the neutron or the appearance of the first theories of the nucleus, Tamm's included. This is the more amazing since the subject was not "Merovingians and Carolingians" but their own scientific lives.

Tamm's report at a Wednesday seminar in the Kapitza Institute was a real event in the life of Moscow physicists and biologists in the late 1950s. He spoke then about relatively recent work by Crick, Watson, and Wilkins and of Gamov's contribution to interpreting genetic code. That his speech was brilliant and topical goes by itself. (It should be stated that Tamm took the floor right after another guest speaker, Nikolai V. Timofeyev-Ressovsky, an incomparable "scientific narrator" whose flair at the rostrum was such that it seemed nobody could compete). His lecture was irresistible because of the pleasant truth and beauty of what he talked about, and Tamm himself obviously enjoyed it. For the audience, the truth was just as exciting as the polemic implications of the whole talk.

On which exact Wednesday did it happen? Which day, which month, which year? My memory has not retained the exact date, and the guesses of others who were there ranged over some three years — from 1956 to 1958; a hundred Wednesdays to choose from (leaving out holidays)! Every step into the past is rather like that. Only archives are reliable. P. E. Rubinin studied the archives of the Institute and found out that it was the 304th seminar on February 8, 1956, Tamm's report was called "Review of Works on the Structure and the Possible Biological Significance of Nucleoprotein Acid".

Another landmark intrinsically linked with the first is also forgotten. And quite interesting, at least from a social and psychological point of view. Actually, it was because of it that I was really looking for the first date.

In the same period, the late 1950s, another seminar — "The Writer and Modern Society" — started up at the House of Writers in Herzen street (one Thursday a month). I found two invitation-cards to meetings with Tamm among notifications of these Thursdays. On Thursday, May 27, 1965, Igor Tamm spoke about laws and properties of elementary particles. I remember somebody shily asked if he believed in quarks himself and

Tamm's circumspect humorous reply: "Generally speaking, I am an atheist but may I give the answer next time?" ... But "next time", Thursday, January 5, 1967, the discussion was on quite a different subject: he spoke, along with Academicians S. D. Skazkin and A. N. Frumkin, about Soviet science in the early post-Revolution years.

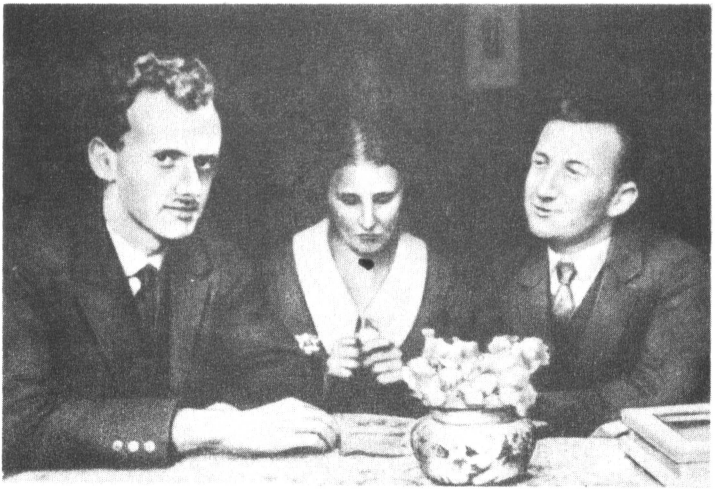
I remember vividly a third Tamm Thursday, this one much earlier; only the invitation has got lost and I have no way of establishing the exact date. But a point of reference has remained: an invitation to a meeting of writers with L. D. Landau on December 10, 1959. The meeting with Tamm definitely preceded it, for to persuade Landau to speak to writers I referred to "Tamm's brilliant lecture & discussion". The argument was rather weak and Landau immediately, and not without a shade of irony, turned the tables: "If writers have already heard from such a good source what modern physics is, what do you need me for?" Fortunately, I was able to object that Tamm had not really spoken about physics...

At that meeting Tamm did mention towards the end that the most striking and important developments were taking place in biology. He went on to say something like the following:

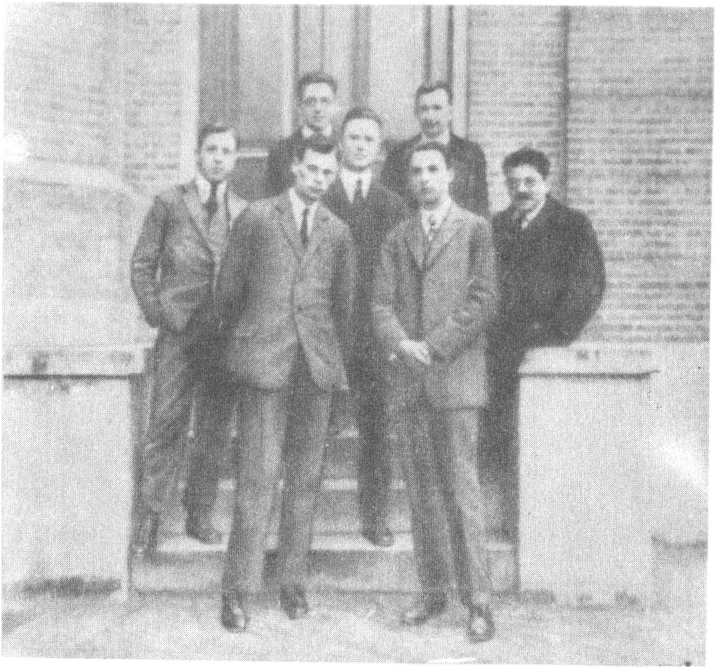
"It is extremely useful for writers to know it. Some of them (excuse me but I am for ever forgetting names) are still lauding God knows what ... It would be rather good if they came."

"They" did not come. Those who were there, and the Small Hall was packed, listened to Tamm like children to a fairy-story teller. I was among them. It all seemed allusively clear and the likely results so far-fetched. I recall a group of essayists surrounding Tamm afterwards by the still unwiped blackboard and its drawing of a double helix and Oleg Pisarzhevski mockingly saying: "I wonder how the poor old anti-geneticists are going to fight the double helix now?" Tamm laughed; "It's indeed going to be hard, but do you want me to tell how ...?" And he told us a virtually impossible story (asking us first to keep it under our hats). This was its gist:

"Soon after the war engineer X. made a simple but ingenious invention that could help save on explosives used in building projects. When a highly representative committee discussed giving him an award, one scientist, undeservedly reputable at that time, unexpectedly objected 'on scientific considerations'. Everybody was, of course, curious to know what these were. And what the man came out with was: 'Where will such construction explosions take place? In the earth! But the earth may get scared and become infertile.' To get round the awkward

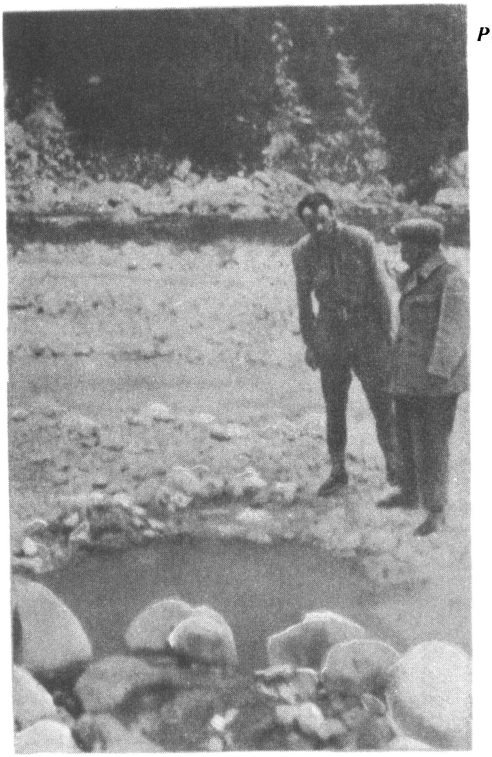


P. A. M. Dirac, O. N. Trapeznikova, I. E. Tamm (Leiden, 1928)

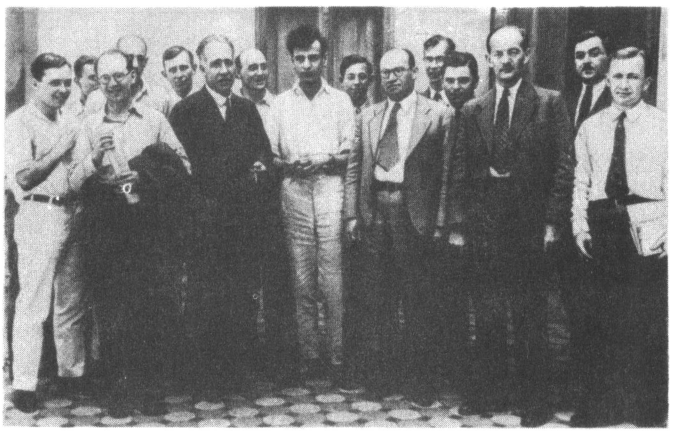


I. E. Tamm in Leiden (1928)

Left to right: Kramers, Govers(?), Rabi, Florin, Kronig, Tamm, Ehrenfest



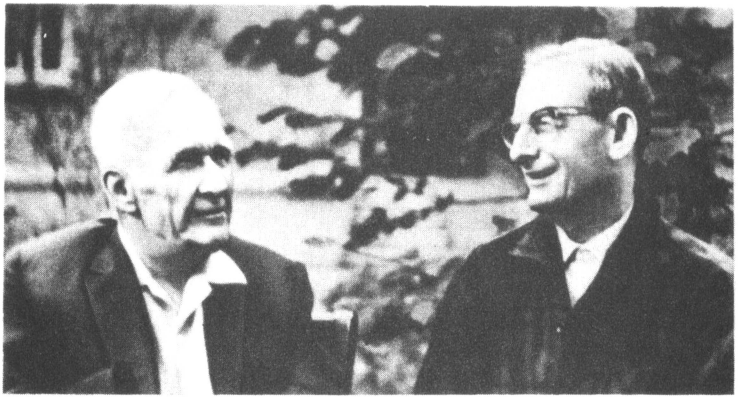
P. A. M. Dirac and I. E. Tamm in the Caucasus (1936)



A Conference on Theoretical Physics (Kharkov, 1934)

Right to left: I. E. Tamm, V. A. Fock, W. Gordon, E. Williams, I. Waller, Y. I. Frenkel, M. S. Plesset, L. D. Landau, V. Crowther (?), N. Bohr, H. Hellman, Y. B. Rumer, L. Rosenfeld, L. Tisza, D. D. Ivanenko

P. Blackett (left) and I. E. Tamm (in the middle) at the Laboratory of Cosmic Rays, the Lebedev Institute (late 1950s)



I. E. Tạmm and Yu. B. Khariton

I. E. Tamm and I. V. Kurchatov (February 1960)

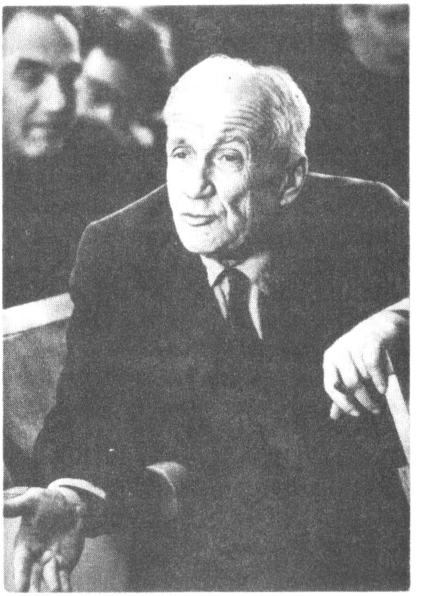
At a reception in honour of N. Bohr (1961)

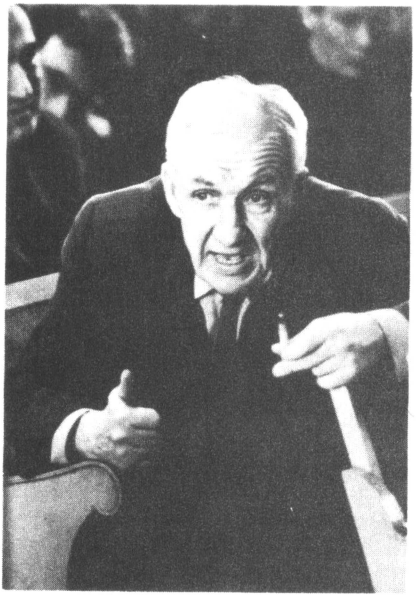
Left to right: Margaret Bohr, I. E. Tamm, A. N. Nesmeyanov, L. D. Landau

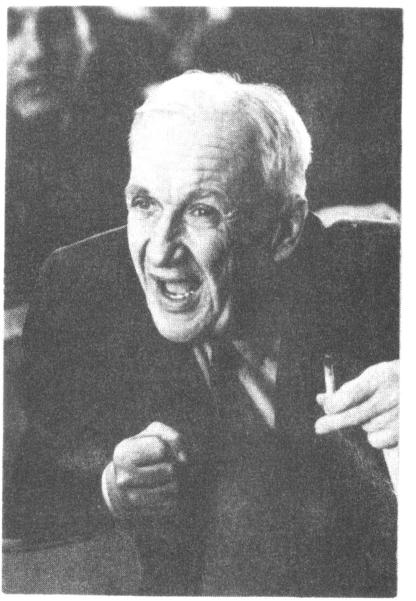
Talking with N. Bohr at the Lebedev Institute (1961)

At the Kurchatov Institute of Atomic Energy (1961)

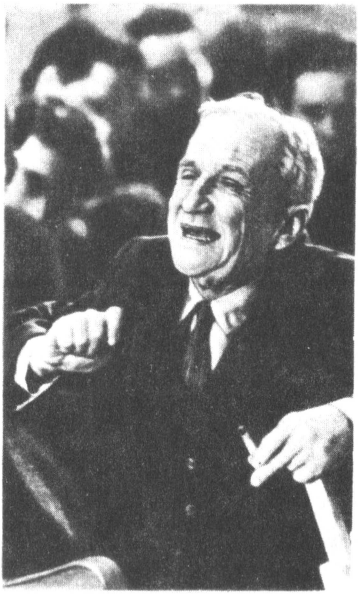
Left to right: L. A. Artsimovich, N. Bohr, I. E. Tamm, A. P. Alexandrov

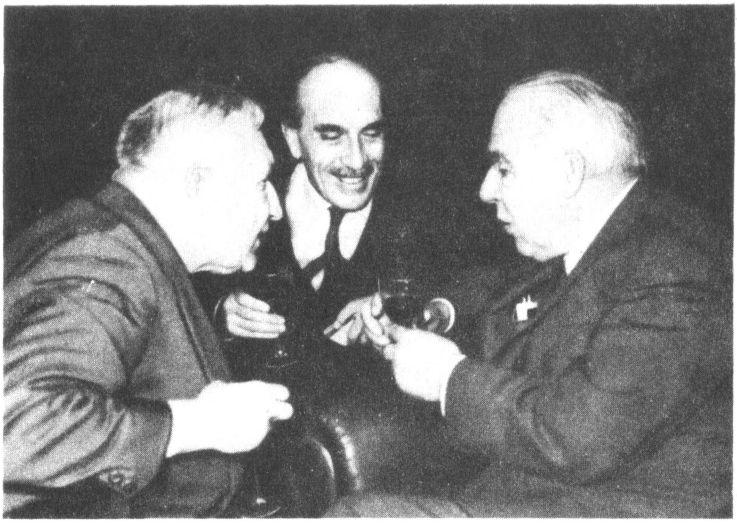






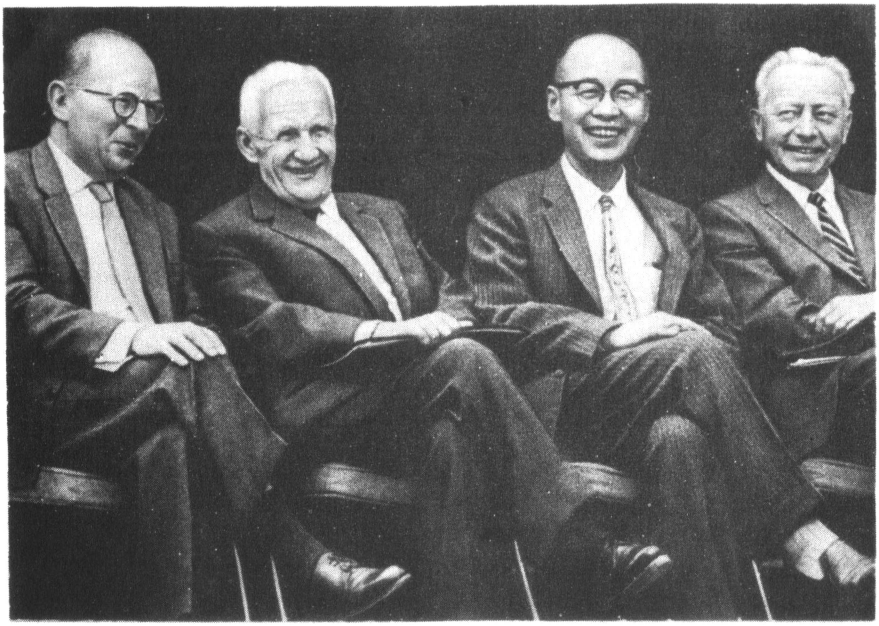
I. E. Tamm discussing scientific problems at a seminar





At the Institute of Physical Problems (1961)

Left to right: I. E. Tamm, N. N. Semyonov, N. Bohr



At a conference marking the anniversary of the prediction of the meson (Kyoto, 1965)

Left to right: L. Rosenfeld, I. E. Tamm, H. Yukawa, G. Wentzel

Discussing a problem at the Institute of Physical Problems (mid-1960s)

Left to right: S. Tsitseika, P. L. Kapitza, I. E. Tamm

situation the Committee decided to put off the matter till the next session."

Somebody had exclaimed: "Igor Evgenievich, you must be joking!" Tamm answered with a bow, "You flatter me," and pointed out that methods of "fighting the helix" were plentiful but it was too late to do anything against "genetics plus physics plus chemistry." He was very vivacious. Even joyful. He asked for questions and answered them generously regardless of the late hour. And again, just as in the old student days, lights were offered for his extinguishing cigarette from all sides — only this time it was clicking lighters rather than matches. I was often to hear the seminar regulars say: "Why don't you invite Academician Tamm at least once more!"

It is certainly not only because Tamm liked and, I suspect, secretly collected examples of witty resourcefulness that he would have enjoyed the little Weisskopf's witty reply to his history teacher's reproach.[4] Dates and events, names and faces sometimes became divorced in his memory too. He was humorously distressed at this.

The disobedience of his memory did not stunt him from telling a lot of funny, psychologically interesting episodes that he had heard or experienced. I do not think it bothered him particularly because he was mainly attracted by the essence of events and the manifestations of human nature while "exact time and place" was not all that vital in his eyes. It is not by chance that different people have different versions of his favourite stories ... Such a "multivariancy", by the way, would hardly have been criticised even by the stickler for the truth Bohr who, according to Stephen Rosenthal, believed that "a good story need not be absolutely spot on; general truthfulness is sufficient."

Bohr was also fond of the following argument which it was rather difficult to disprove: "But, my dear friend, if a truly interesting story is to be told there is no need to stick too strictly to the facts." In Tamm's case, however, everything was more subtle: he liked to stick to the facts and not to invent them for effect. Yet his memory and lively imagination created versions that were not strictly alike. He sought accuracy in his telling but never got upset if he went a bit astray so long the gist was right.

Here is an almost word-for-word account I have (and, I think, quite a model one) of one such story which I wrote down right after meeting Tamm:

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[4] The proceedings of the Brooklin Conference with Victor Weisskopf's paper was published only in 1972 when Igor Tamm had already passed away.

"Oh yes, here is something else about Dirac. It also happened in Cambridge, or perhaps... No, no, definitely in Cambridge. Heisenberg came there to address a seminar in Trinity... Just a minute, wasn't it the Physics Society? Or the Kapitza Club? No, I believe it was Trinity College... He spoke about some recent work of Heitler or Pauli. But that doesn't matter ... You see, Heisenberg spoke very confusingly, explaining this by his not having his notes with him. Then followed a heated discussion, all rather muddled and unclear. When all were leaving someone asked Dirac what he thought of Heitler's arguments ... Yes, of course, Pauli has nothing to do with it ...

" 'I don't have to think. I know the idea in this paper,' said Dirac, 'Heitler himself has told me about it.'

" 'Why didn't you say, Paul?'

" 'But nobody asked me'.

"This is what Dirac was like. You know, once in Cambridge he and I were walking, having a lively conversation; later a physicist (I don't remember who) came to me and said that he had seen us and could not help feeling amazed at seeing a 'talkative Dirac', not an everyday sight."

In the autumn of 1960, during a long walk in the mountains near Gagra (on the Black Sea) where the steepness of the narrow paths and thinness of the air subdued even the youngest of his companions, Tamm seemed unaffected; I heard so many stories from him about physics and physicists that I couldn't help exclaiming: "Good God man, all this will presently be forgotten and you just won't write your memoirs!"

His reply was instant and disarming:

"You mean to tell me that my face shows that it's time for me to start writing memoirs?"

He had only just celebrated his 65th birthday, but I do not think he believed that old age with its inevitable illnesses would creep up on him. This attitude was personified in his cowboy shirt with rolled-up sleeves, in his ceaseless smoking, in his passion for chess, and in his almost daily mountaineering. For him, a born climber, it was child's play. Questioned about his climbing experience, he asked every new listener from Writers' House:

"But do you know what mountaineering is?" And, interrupting some lengthy, boring explanation, he would say:

"No, no, the shortest definition I know of is that it is not the best way of weathering the summer!" And he would laugh heartily with the rest.

On the day of his departure he refused a lift:

"Oh no thanks, I want to go by helicopter. That's something new!"

Accompanying him to the helicopter pad on the beach, I reached for his light overnight bag, but he stopped me and, smiling, once again said:

"Does it show on my face that it's time for someone to carry my bag too?"

That autumn at Gagra I read through a lot of material for a book about Rutherford and every now and then told Tamm something of what I had read as if earning his stories on the subject. But I was very disappointed when Tamm, who would willingly talk about his time in Cambridge in the early 1930s, said regretfully that he had virtually no personal memories of Rutherford. But, by way of compensation, he added out of the blue:

"Things will be different when you start writing about Bohr."

"Bohr???"

I was so startled that I figuratively put three question-marks to the question. Rutherford's biography, which was really only at the planning stage, was daunting enough without anything else. Anyway Bohr didn't as yet belong to history — he was still very much alive and kicking!

"Is that an obstacle to a book?! Oh, no, you will, you will write it," Tamm waved away all doubts with a mysterious confidence, and thereby mapped out the literary life of his once negligent student for several years to come. And, as it turned out, he was right.

Eight years later, when the time came for me to start asking Tamm about Bohr[5] "pencil in hand", rather than casually I reminded him of that prophesy in Gagra. Tamm looked genuinely puzzled:

"Did I say that? My memory really must be slipping."

He went on to explain that he had already heard how he "some time, somewhere, in something" had been very shrewd, that people had remembered it and he had not. So when he was reminded, he felt awkward, as if he had been praised and had not deserved it.

Naturally, I used every opportunity, even "on the move", to get Tamm talking about Bohr. But in 1968 and 1969 such occasions presented themselves less and less often, for unfortunately he was already gravely ill. Our meetings, always agreed in advance through someone close to him, often had to be cancelled. I remem-

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[5] I am not citing here Tamm's Bohr stories. The most interesting of them can be found in my book *Niels Bohr*.

ber being very sad in 1968 when I learned that Tamm was doomed to a respirator for the rest of his life. The memory of the recent torturesome death of Landau was so fresh that the very thought of Tamm too being doomed sharpened the tragic sensation that the giants were dying off.

I last visited Tamm in his flat on Gorky Embankment on March 7, 1969. When I got home, I put everything down in detail (I had a premonition that this visit would be the last). It only remains now to quote a few passages from that record.

He lay on a high bed wearing striped pyjamas, the respirator fixed to the medical collar. It all looked as though the machine was graciously keeping him on a loose rein, but still on a rein, so that this agile and freedom-loving man would not escape. This impression was yet oddly comforting, for it smoothed over the picture of his helplessness. This impression was rooted in his earlier agility and liveliness, in his earlier cordiality, in his earlier readiness to smile; in all his earlier features which had not yet left him and were still written in his pale, strong, deeply wrinkled face. It was only the boniness of his hands, perhaps, that aroused the sensation of something irremedial. However, I did not notice that immediately but only in the second hour when we sat down to play a game of chess at the desk in the next room.

He was helped there by a young fellow, a medical attendant in a white robe, silent and humble like a lay-brother in an abbot's cell. The youth followed Tamm, taking over from the machine, compressing and extending manually the red rubber bellows. The short move exhausted Tamm and he breathed with greater difficulty at the table than in bed. Yet it was impossible to dissuade him. "... I am deprived of everything — of climbing, skiing and walking; all I've got left is chess ... You know Feinberg always used to have an edge but now we have evened up ..." He said it with the full trustfulness of a child, with no thought that his devoted friend and partner might simply not want to deprive him of these last tiny victories. It was then, at the chess-board, that I noticed how much his hands had changed: he could only move the pieces by pushing with weak fingers.

... Warned that he tired very quickly, I had not even thought of playing chess; I had intended to ask him two or three questions to clarify some points in his stories about Bohr, to entertain him a little with my interesting finds of sources for the history of quantum physics in the Copenhagen Archives, to pass him good wishes from Mrs. Margaret Bohr and Leon Rosenfeld, and then to say "good-bye, see you soon". But our conversation dragged on. His interest in the archive on Bohr was insatiable and instead of

asking questions I was the one that answered. On hearing that Bohr's science correspondence comprised 6,000 letters, Tamm with almost natural rapidity, cut through by his noisy breathing, started calculating aloud:

"You may be overwhelmed, but he had a long life... some 60 years in science. Am I right? A hundred letters a year is two a week... You know, I think the density curve of his correspondence by years would be stunning. It would reflect the development of quantum physics ... because ... because everybody needed Bohr in those troubled times ..."

He asked me with a pale smile: "Carry on, do go on ...", and he fell silent. I began to tell about the most unusual treasure of the archives — 175 lengthy interviews taken by historians from 95 veteran physicists of the 1920s and 1930s. But soon I understood that, as he listened, Tamm was thinking about his own long years in science. Suddenly he said with no obvious connection to what I was telling him:

"You know, I fall to thinking just how lucky I've been in this life. Take my pupils and colleagues at the Lebedev Institute — what lovely people they all are both as scientists and as human beings! You are acquainted with ... (He gave names) but the others too, the others too. I don't think there was ever any bad blood between any of them. And no ..." (He added a not very decent word).

On taking leave I gave him a parody of *Faust* in German, a manuscript of a comedy written by Bohr's collaborators, which had been presented to me at the Bohr Institute. He began to leaf through it right away, stopped at the cartoon portrait of Ehrenfest and exclaimed in a low voice: "Now that is a very fine man, and he ended so tragically."

Six years later, in the spring of 1975, again working in the Copenhagen Archives, I decided to make a copy of the correspondence between Bohr and Tamm. There were six letters, three each way, in a thin file, all from the mid-1930s. The earliest was dated June 1934. It took me back more than 40 years, to the occasion with which this piece began: to the Bohr-the-lecturer and Tamm-the-interpreter duet at the University and the Polytechnical Museum. The past was alive in the letters.

Marx str.,
Moscow, 66

Dear professor Bohr,
I bet to excuse me for having delayed for so long sending you my notes of your Moscow lecture. They are very cursory, with many lacunae, the completeness of different parts out of proportion with their respective importance. Many passages were noted by me in Russian and are translated backwards in English. In short I doubt that they may be of any use to you...

I hope very much to hear soon from you that you finally decided to come with your son to Caucasus and I am looking forward to meet you here this August.

With best regards to Mrs. Bohr.

Yours very sincerely Ig. Tamm.

Universitetets Institut for
Teoretisk Fysik
Blegdamsvej 15,
KØbenhavn Ø.
Den...June...20th...1934

Dear Tamm,
I thank you very much for your kind letter and all the trouble you have taken in preparing the notes of my lecture in Moscow which give a good idea of the general contents and tendency of the lecture. Our journey in Russia was a great experience to my wife and me, and we are both very thankful for all the kindness you showed us. I look forward very much to come back on another trip and especially to go for a walking tour in your mountains with you...

With kindest regards to you and your family and all common friends in Moscow from my wife and me.

Yours, Niels Bohr

For a brief minute these two short epistles brought both Bohr and Tamm back with all their love of life, kindness, softness and strength.

V. A. Engelgardt

THE WAY HE REMAINS IN MY MEMORY

Popular wisdom as expressed in proverbs and sayings has it that to know what someone is worth, you've got to go through thick and thin with him. I may sound disrespectful of folk wisdom, but I think it may err now and then.

To begin with, the time required for real acquaintance depends on the person you are dealing with. The saying may hold true for commonplace, ordinary people, the bores. Here a good deal of time is needed to gauge the man, to learn what makes him stand out from the crowd. But if he is an original and brilliant personality, even a fleeting acquaintance is enough — his words, his deeds and his soul will be eloquent: you may feel strongly drawn to the man and ready to sacrifice anything for him, or just the other way round, you may feel utterly estranged.

Secondly, and this is the main thing, there exist special condi-

tions under which human character is revealed. I am deeply convinced mountain-climbing is one of them: a unique combination of inevitable hardships and deprivations with the enchanting beauty of mountain peaks is an experience a hundred times more illuminating that the common rut.

This fully applies to my friendship with Igor Evgenievich Tamm.

Fate willed it that our first two meetings — one very short one, the other a bit longer — took place at least five or six thousand feet above sea level.

I recall our first encounter. I think it occurred in the early thirties, on the north slopes of Mount Elbrus. Our small party moved into the mountains toward the ranges of the Western Caucasus, along pathways little trodden in those times. And in some wild and desolate place two mountain-climbers descended toward us, with sunburned faces, tired but in cheerful spirits. I came over and found that the men I talked to were physicists whose names I knew well but whom I hardly expected to meet out in the wild.

They were Igor Tamm and the outstanding British scientist Paul Dirac. Dirac had come to the Soviet Union to attend a scientific conference and, with a few days free, accepted Tamm's invitation who shared the same, "mountain-climbing hobby" — their second ruling passion after devotion to physics.[1] Tamm had no difficulty in talking Dirac into trying the northern face of Elbrus. And I encountered them as they were returning from that thrilling trip. Tamm later regaled us with a lively description of all the peripeteias of their joint ascent of Europe's highest peak — and they combined this thrilling experience, in their rest hours, with no less fascinating excursions into the highest domains of theoretical physics.

In science our interests were far apart. I do not think I can recall any contacts with him in that field. But as good luck would have it, we came across each other again in the mountains. That was in 1947. My age was beginning to tell, but I still felt like breathing the air of glaciers and alpine meadows and — what was most important — to instill that love in my two daughters. We set out for a mountain camp of the Nauka sport society located at Alibek, over the Dombai meadow. We lived in a tent. A little over to one side, there stood a small hut where several scientists

[1] Actually it was Tamm who "infected" Dirac with it. Dirac used to go to mountains only with Tamm — *Ed.*

stayed: Igor Tamm, Boris Delone, the mathematician, and a few others.

The Russian saying has it that to get to know a man really well you should eat a pud of salt with him (pud is 35 pounds). But we did not need that much — a few pinches thrown into a messtin with our hot supper was enough; evenings, sitting around the bonfire and chatting, we learned quite a lot about each other — and we struck up a friendship. We admired Tamm for managing difficult climbs on a par with the young. I remember particularly well an assault on one of the most difficult peaks in the Amanaus range. We down at the camp were consulting our watches in some anxiety as the deadline for their return had just about expired and they were nowhere in sight. But then the first figures loomed in the distance and then all the file came into view, and soon after the tired but content party told us all about it. There had been moments that required courage, skill and fortitude.

Thus I became acquainted with Tamm the climber before I came to know him as an outstanding physicist, a clear-minded scientist with a broad range of interests. He had an astonishing ability to grasp matters far beyond his field and get them across to a mixed audience. It was from Tamm that many a Moscow scholar and natural scientist heard the first clear description of the genetic code principles — a new branch in natural science and biology where we are witnessing brilliant accomplishments. Tamm spoke out against attempts within the Academy of Sciences to foist views which were at variance with the best interests of science. No doubt Tamm's immense scientific prestige and his high-minded moral make-up contributed to the rehabilitating effort which, in a short space of time, helped to overcome the Soviet lag in many key areas of biological research.

So, my memory retains three images of Tamm in health and strength: in the hills by a fire or checking the crampons on climbing boots; chalk in hand in front of a blackboard in a crowded hall in the Atomic Energy Institute, and at the rostrum during a general Assembly of the Academy of Sciences. He was first-class everywhere: climbing mountains, at the pinnacles of international science, in the family of Nobel Prize winners, at the apex of moral and ethical standards.

But my memories take on sombre hues as I turn to a period when Tamm's strength was sapped by an ominous malady which led to his premature and tragic end. One day we landed in the Academy of Sciences hospital — our wards were next to each other. I had some passing ailment, while Tamm was suffering the initial, not clearly pronounced symptoms of a disease which progressed

relentlessly to carry away his life. There was already the hard wheezy breathing that left him bedridden for the rest of his life.

Some other man would have lapsed into physical and moral prostration and lost any interest in all beyond immediate concern for his health. Not Tamm! He would literally snatch out of my hands the latest scientific journals which I brought from the Istitute, he would react to whatever was new and interesting in scientific life, and discuss with abandon things from the realm of biological studies which were making rapid strides in this country. Our hospital talks are imprinted in my mind as a vivid image of a doomed, but manfully defiant Tamm. My meetings with him at a later stage in his illness are even deeper impressed on my mind. By then he was back home and chained to an iron lung. Visiting him, I saw him surrounded by pupils and colleagues with reams of manuscript and graphs.

The sad memories relating to the final period of Tamm's life add another touch to his vivid image. The image is of a living Tamm who has a blend of dynamism and a cool head, abiding moral uprightness and a kind heart, and an ability to give himself up completely. A man endowed with such wonderful qualities ought to have lived and born more fruit!

I. L. Fabelinsky

A VIEW FROM THE SIDELINES

The contributors to this publication are Tamm's friends, pupils and close associates. I would not go so far as to place myself even among the last category.

I heard Tamm's lectures in my student days, and for many years we worked at the same research institute. To me he was a benevolent and charming person, an extraordinary man. But the same could be said by hundreds, indeed many hundreds. Hence I felt that I would be better among the readers. However, some of Tamm's pupils and friends have prevailed on me that the views of an outsider might be of interest. Perhaps this is so.

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In 1934, we physics students were waiting in the "new" building of Moscow University for Professor Igor Tamm, the head of the Chair of Theoretical Physics, to start a quantum mechanics

course. For just about all of us this was our first sight of him, although we knew his *Fundamentals of the Theory of Electricity,* which even today is one of the best textbooks on electrodynamics. We were studying it with another professor. We had likewise had distinguished professors for general physics and mathematical courses. Our concept of what the man who was about to make his appearance would look like was largely predetermined by what we had seen before, and above all by the Book.

He was quite unlike what we had either seen or imagined. A short man shot into the auditorium followed by sparks from the cigarette he had left outside. He dashed across to the wall opposite, came back, stopped half-way between the door and the wall, and turned to the students with a lively, smiling face. Thereupon he started on a breakneck introduction to the course pacing about the auditorium. He would pause at the blackboard for a burst of formulas — the quantitative expression of what he was saying — followed by exposition on the physical essence of the matter.

Tamm did it all with so much enthusiasm and inspiration that even those who could not grasp the half of what he said were not left apathetic. Everybody felt that they were witnessing something out of the ordinary. We were quite caught up. Tamm backed his words with gestures to emphasise a point. In fact, his whole being was involved in what he was saying. When he had covered the whole blackboard with his scribbles, except for a small patch at the top that was out of his reach, he would hop to write a letter or add a stroke. His lectures were anything but prim and academic. His speech was rapid and full of intonation. His ideas were always clear and unambiguous, although he sometimes left his sentences unfinished. His delivery of the subject matter was impassioned and convincing.

He did not attempt to smooth out sharp edges or gloss over contradictions and was delighted when he was able to give a convincing explanation. His face shone with joy at such moments. We looked forward to the next lecture as to a new instalment of a detective story.

The two-hour lecture was interrupted for a break with everybody filing into the corridor for a smoke. Tamm was a heavy smoker and he used the break to have a smoke rather than to relax. At such moments he was always surrounded by students and tutors who had classes in adjoining auditoriums. Tamm always did the talking. Sometimes he explained something the students had not fully understood in the lecture, but more often than not he would extol mountaineering, adding that he could not understand anyone being able to live without it. On occasion,

when he did not happen to have a "badly needed" cigarette, he would ask one of some delighted student (Professor Tamm took a cigarette from me!) and he always offered his cigarettes if he thought he had seen some student smoking earlier who was not smoking now. Tamm was absolutely open to his students, and his personality invited communication from them. He never gave us to think that we were asking "silly" questions an eminent professor.

But, of course, there was not a shade of familiarity, not with a man we so admired. Our group was especially lucky in that Tamm also conducted seminars, which was a fascinating experience.

Before Tamm began his course of lectures we heard from senior students that quantum mechanics was an unusual, and totally incomprehensible discipline, and that whatever could be understood was, in fact, a misunderstanding. Everybody was afraid of it, especially those who had no real calling for this science. How then did quantum mechanics come to be so unexpectedly fascinating? Of course, Tamm was a marvellous lecturer, but I don't think that was all. More than a highly qualified physicist and excellent exponent of the fundamentals of his discipline, he was an active creator of science, a man of wide interests and broad outlook. But we never once heard him say "I have done this" or the more modest "We have done this" or anything that even hinted at personal achievements.

As I did my first research, Tamm's fundamental work *A Quantum Theory of Molecular Light Scattering in Solids,* published in 1930, became part of my scientific "Bible". The theory has retained its relevance and is still incorporated in the university theoretical physics course. It was in it that Tamm introduced the concept of the energy quantum of elastic oscillations later named the phonon by Y. Frenkel. The work gave rise to the branch of physics now called quantum acoustics.

At that time and in subsequent years Tamm devoted much energy to a theory of nuclear forces which invited reproach from some people ("He doesn't know what he is doing and is trying to pull others in"). The atom bomb dropped on Hiroshima in 1945 put an end to all such censure.

In 1936 or thereabouts the Division of Physics and Mathematics at the Academy of Sciences met in the old building of the Lebedev Institute in Miussy. Frenkel presented a paper on his latest research. Tamm took exception to the principal tenets of the paper. At first he asked caustic questions, then launched an all-out attack. Frenkel held his ground, while Tamm worked himself up to a point of snarling at the blackboard and the speak-

er, to the point that, I found, what he was saying was offensive and even insolent. The Chairman clearly could not cope, and the meeting ended without agreement. I was not sure who was in the right, but one thing seemed quite certain, that being that there were no more bitter antagonists in the world than Tamm and Frenkel. This was all very unfortunate because we knew Tamm was a nice person and we had heard many commendable things about Frenkel. And if, as is said, "thy soul blossoms on thy face", then such praise was due. And so it was that two nice men and eminent physicists had a bitter quarrel. For a long time afterwards I seriously believed they were enemies.

I came across Tamm again many years later. He had been a staff researcher at the Lebedev Institute for many years and I had just joined. In the late spring of 1949 Tamm told me over lunch at the canteen that he was going to the Chaika holiday home at the seaside near Riga. It so happened that I too was going there, only about ten days earlier. Tamm wondered whether I could not arrange with the manager to set aside a quiet room where he could do a spot of work undisturbed. The manager said of course he would reserve the best room for Tamm. However, when Tamm arrived and we went to see the man in his office, he said he could not spare a bed, let alone a room, although he assured us that his promise was still valid. He suggested Tamm find a place to stay for a couple of days, by which time a room would be vacated. I was speechless with consternation, but Tamm laughed and said: "All right, let's go and look for something somewhere else."

The hotel was full up, and when I suggested Tamm might put up in my room, he abruptly turned down the offer, although he agreed to leave his belongings there. The situation was pretty depressing, but Tamm was cheerful, even in high spirits. As he glanced at me he said: "It's all right, there is a good friend of mine staying in a private house somewhere near here. I am sure that between us we'll be able to come up with something."

When we found the house, the smiling man who came out to welcome us turned out to be none other than Frenkel. I stood dumbfounded as the men hugged each other and Frenkel invited us in. I discovered that they were close friends, and had been long before the incident that I described. Frenkel's son even recalls his father once telling Mrs. Frenkel that in the event of his death she could absolutely rely on Tamm.[1] Neither Frenkel nor Tamm

[1] *Recollections about Y. I. Frenkel.* Leningrad, 1976, p. 243 (in Russian). See also V. Y. Frenkel's contribution in this book — *Ed.*

doubted they would stay friends: their personal relationship and their scientific views were simply kept apart.

Tamm often turned up on the volley-ball battlefield in the Institute's courtyard in Miussy. He displayed a good deal of temperament, and when he fumbled at a critical moment, his sense of guilt was overwhelming and he gave vent to emotion by gesticulating vehemently. When he made an especially "tragic" blunder he squatted on the ground holding his head between his hands and swaying from side to side.

In the winter vacation, many of the staff of the Institute went to the Abramtsevo holiday home near Moscow which belonged to the Academy of Sciences in those years. There they would often go skiing in large parties, Tamm always demonstrating thrust much younger people were unable to match. He took particular pleasure in going downhill.

His favourite spot was on the opposite side of the River Vorya from the holiday home. It was a hillock some 100 feet high with a fairly steep slope facing the river. Between the river and the slope there was a narrow patch overgrown with thin scrub. Clambering on top Tamm would go down full pelt. He was not particularly good at making turns and to avoid finishing on the ice of the half-frozen river, he would fall over just before the scrub. Since he was going all out he would promptly be buried in the snow.

Once, when he was trying to bring himself to a halt, he plunged into loose snow and was completely submerged, with only the skis sticking out. Naturally, I rushed to help him out, but met with resistance. I thought he had broken a leg or something and was in pain, which encouraged me to redouble my efforts. When finally he was able to get out of the snow, he gave me a good dressing-down. "What did you go doing that for?" he inquired. "Couldn't you see I didn't need any help?"

An engineer from a Moscow factory usually joined us. He was a good skier and, unlike me, did not have to fall on his side or go down the hill at an angle. As Tamm went up while the engineer and I remained at the foot of the hillock, the engineer asked who was that fearless man who handled his skis so badly. I told him. When we were going back for lunch, the engineer caught up with Tamm and asked him whether he was a relation of the famous slalomist Evgeny Tamm. Tamm smiled all over his face and said Evgeny was his son. He was very proud to have been identified through his son, not the other way round as was normally the case. It gave him great pleasure to recall that incident.

Quite often terms denoting degrees of difficulty of climbs

cropped up in Tamm's facinating alpinist stories. Once I asked him to explain the meaning of 5B. He smiled and asked whether I could imagine the difficulties involved in creating a general theory of relativity. I said that was outside the scope of what a human normally could do. I suggested it must be incredibly hard, something beyond my imagination. "There you are," said Tamm. "5B is much more difficult." He grinned and said that he had never attempted 5B.

Tamm was always having intriguing ideas that might lead to unusual discoveries, and not necessarily in science. He was optimistic about finding the abominable snowman. Although he did not take part in the search, he was sure the snowman could and must be traced; he saw it as one of the greatest challenges. The eye-witness accounts seemed quite convincing, but, most probably, they simply were consonant with his longing for the extraordinary.

Each year some pupils and friends of the late G. S. Landsberg got together in the Landsberg flat on the anniversary of his death (February 2). On one such occasion Tamm brought up the subject of the snowman at a time when the search had reached its peak. Tamm's words suggested it would be found in a matter of days.

One of those present, who was more of a pessimist, declared that the snowman did not exist, which threw Tamm off balance. "The snowman will be found before the autumn is out," he retorted. "It's a bet." To which the other man replied: "The snowman will never be found, I'll take you on."

They made a gentleman's bet. In a couple of years Tamm must have realised that he had lost. He never admitted it. He just no longer talked about the snowman. He hated to be a loser or to have to abandon a beautiful dream.

Tamm had wide-ranging interests in physics, and not only in physics. Once (I think it was in 1964 or 1965) he asked me about application of laser sources of light in the spectroscopy of scattered light. I said the results were very encouraging, especially in the study of spectra where the lines crowded together and could not be resolved due to a large width of the exciting spectral line. Tamm asked me whether he could see a picture of the phenomenon.

When I saw him again, I showed him a photo of the fine structure of Rayleigh's line, with the Mandelstam-Brillouin components clearly shifted from the central line and standing out as if drawn in ink. The photo impressed Tamm: it was an illustration of an effect whose quantum mechanical theory he had formulated back in the 1930s. "Have you got many of these?" he

asked. I said very many. He asked me whether he could keep the photo. When I said yes, he was delighted. He asked many technical questions about the method and its possibilities.

All that was good was a source of delight for Tamm. He was one of those people few and far between to whom a stranger's success was just as pleasing as their own or that of their students.

Everybody, even those with whom Tamm was merely on nodding terms, was taken by him.

Another thing about him was that he never allowed anyone to help him on with his coat. He acted so swiftly that the would-be helper was still standing with an outstretched arm when Tamm had already disappeared from view. However, there was one exception. In one of the last winters of his life, when he had almost lost the use of his right arm, the Institute's veteran cloakroom attendant Matrena Ivanovna scored total victory over him. "You are not the boss here," she declared. "Here I decide what goes on," and helped him into his coat. There was nothing Tamm could do except submit.

V. Y. Fainberg

A PASSION FOR SCIENCE

I had the good fortune to work with Igor Tamm from 1950 to 1971, that after him being my mentor earlier, and I feel I can say he did a lot to point me in physics, to help me form my world outlook. Even those who knew Tamm very closely will find it impossible to give a full three-dimensional picture of him. Taken all together, the reminiscences of his friends, colleagues and relations may just be able to give some idea. I set myself the modest task of retelling a few episodes illustrative of his style of work, attitude to physics and physicists, and ethical principles.

In 1947 when I saw Tamm for the first time, I was a fourth-year student at the Moscow Institute of Physics and Engineering. His lectures were on the theory of relativity. I remember my first impression of him: short, agile and grey-haired. He did not seem to change much in subsequent years. He spoke animatedly, but clearly. We liked his lectures. I recall him repeating time and again that Einstein was a genius because in devising his theory of relativity he, unlike most physicists of the day, was quick to comprehend the sterility of attempts to explain the Maxwell theory

of electromagnetic waves within the old concepts of ether and identified the only correct path.

When I came to work at the Theoretical Department of the Lebedev Institute upon my graduation, I took up applied problems under Tamm.

In those hard times the focus was on applied problems. A lot of discipline and solid work was called for. What pleased me from the very start was the spirit of comradeship and equality. Tamm was held in deep respect but that did not stop him being simple of manner. Simplicity, I came to realise, was the fruit of a democratic outlook. His qualities were behind the good team spirit at the Theoretical Department.

I remember clearly our work with Tamm on strong interactions — nuclear forces and the isobar theory. His profound intuition and general elan came into play. Fundamental matters always held great attraction for him. His dream was to evolve a consistent theory of nuclear forces and elementary particles.

His first major works on the theory of nuclear forces appeared in 1934. In 1945 he devised a method of solving equations for strongly interacting elementary particles (the Tamm — Dankov method) that was not based on the perturbation theory. Early in the 1950s quantum field theory, and particularly quantum electrodynamics, went through a stormy second youth. The first experiments were performed on π-meson scattering off nucleons (Fermi, 1951). These events inspired Tamm to a fresh effort at drafting a consistent theory of strong interactions.

I remember him making an "all-hands-on-deck" plea (which he launched in 1952 after one of his seminars) for a theory of nuclear forces. Such a theory, he argued, was both possible and necessary. It would open up new opportunities given the new advances in quantum field theory. Tamm urged all to buckle down to that problem. V. P. Silin and I immediately responded. To begin with, we perused Tamm's cornerstone article about the Tamm-Dankov method[1] (he called it "truncated equation method"). Going through the calculations in the article, I obtained a different formula.

I checked and double-checked by calculations, but the results were the same. I was certain that there must have been an error somewhere, so I rang up Tamm and told him about it. He replied calmly it was most unlikely since all the formulas had been verified many times, but asked me to come over right away. I did and

[1] I. E. Tamm. *Collected Scientific Works*. Vol. 2, Moscow, Nauka Publishers, 1975, p. 100 (*J. Phys.*, 1945, 9, No. 6, p. 449).

Tamm had a look at my calculations, took a clean sheet of paper and silently transformed my formula — and fairly fast at that. To my utter amazement, the final result concurred with his. Even worse, mine was the more cumbersome formula. So I had to eat humble pie. But he did not exult in his triumph, saying jokingly that both of us were right. It was a good lesson to me; I became more cautious when I compared my results with Tamm's. It became absolutely clear to me about a year later that he always calculated very fast and virtually never making mistakes. His ability to reduce formulas to a most compact form stunned me.

The first step Tamm, Silin and myself made was to try to prove that the meson theory leads to a bound state of the proton and neutron, i. e. to deuteron stability. The calculations were made according to the Tamm-Dankov procedure. Even by present standards, our calculations were rather awkward. For that matter, we made independent calculations in "three hands", so to speak, and we would meet mornings two or three times a week to assess progress. It was instructive and interesting, albeit difficult for me and Silin. We computed non-stop day in and day out, working ourselves into exhaustion so as not to fall behind Tamm. Perhaps only the isobar theory (much later) has impelled me to work (again with Tamm) with such great abandon.

Tamm would always come to us bright and cheerful; but Silin and I had a sense of inferiority. On one occasion, when we were really low, we asked him how much time he spent on calculations and how he managed in general to work and reckon so fast. His reply amazed us: "Unfortunately, my working capacity has gone down in the last years; I remember I could keep going for two days and nights on coffee, but now I get as tired as hell after a single day and night." Well, he was 57 then and Silin and I were only 26!

We felt none the better for that, of course. Our torments continued. As our final formulas at long last evolved and deuteron stability was proved, they turned out to be twice as long as Tamm's. I derived some solace from the fact that my results were the same as Silin's. But we well remembered what had happened with the formulas from Tamm's article of 1945. We knew we faced a hard job "truncating" our results; yet we could not do it fast.

Tamm suggested that we get down to our article, and he took the trouble of doing the first draft. That was good news for Silin and me: we hoped we would be able to put our formulas into shape in the meantime. However, the final version of the article was soon ready. So we had to devise some stratagem: we told him that our formulas were "nearly" identical, and that the article could be offered for the press. Our results were also correct

after all! But every cloud has a silver lining: it took some time to get the article through. While it was checked and double-checked, we managed to bring our results in line with Tamm's. I think he guessed our ruse.

Asked the secret of his knack of reducing final expressions to a concise, manageable form, Tamm used to say it was only a matter of selecting independent variables, designating them properly and expressing everything through them. That allegresse in tackling involved problems was a hallmark of his talent.

In 1953 Tamm advanced the bold hypothesis that a nucleon could be brought into an excited ("isobar") state with a spin of 3/2. This assumption was prompted by the Fermi experiments on π-meson scattering off nucleons. Tamm was much taken by the new idea and many "old-timers" will remember the isobar boom (which inspired S. Z. Belenky to verse).[2] Tamm rallied a cohort of volunteers and infected them with his enthusiasm.

Tamm's perseverance in striving towards his goals was astounding. Today, twenty years after that time, short-lived excited states (resonances) have so firmly established themselves in the parlance of physicists that no one even bothers, in effect, to recall the pioneer. But to the pioneer, the objections came thick and fast, even from acknowledged theorists. It was deemed "a naive understanding" of excited states.

Tamm stuck to his guns. The calculations done according to elementary perturbation theory (account was also taken of attenuation) agreed miraculously well with experiments on scattering in P-state[3]. Yet the difficulties involved in describing scattering in S-state (unresolved even now) dampened his enthusiasm. This did not stop him though in subsequent years frequently drawing on isobar concepts to describe processes involved in strong interactions. The isobar theory calculations brought into play Tamm's ability to evolve fast, from general group theory arguments and considerations on invariance, expressions for the generators of the group. He did not need to consult reference books.

Tamm was always for bringing his calculations down to numbers and comparing the results with experimental data. He was a deft hand with the slide rule. When the slide rule was not up to the job, calculating machines and even electronic computers were brought in. The problem was we did not know how to use them properly. I recall one incident. Late in 1953 Tamm became

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[2] See E. L. Feinberg's reminiscences in the present collection — *Ed.*

[3] I. E. Tamm. *Collected Works.* Vol. 2, p. 136 ("Zhurnal experimentalnoi i teoreticheskoi fiziki", 1954, Vol. 26, p. 649).

interested in the "New Tamm-Dankov Method" suggested by Dyson. Tamm spent much thought and effort on searching for a consistent method of computing strong interactions without perturbation theory. The Dyson idea of "physical vacuum" appealed to him.

Silin and I also engaged in applying the new Method to π-meson scattering off nucleons. We soon had integral equations, but solving them analytically was impossible. And due to the introduction of "physical vacuum" into the theory, spurious poles appeared in the scattering amplitudes which had to be excluded from the equations. We felt we had found and excluded all such "non-physical" poles. But even then only a computer could solve the equations. The electronic computers which this country had in those days could be counted on the fingers of one hand, and all of them were booked up with applied problems. Tamm came into his own in such circumstances. He had the ability to infect with his ideas and faith.

He approached M. V. Keldysh, Director of the Institute of Applied Mathematics (IAM), and his associates and requested time on their "Strela" (Arrow) computer. Tamm made an ardent speech to IAM leading researchers with Keldysh at the head about the significance of a consistent theory of strong interactions. Keldysh always had great respect for Tamm and a special group was set up there and then under the distinguished expert M. R. Shura-Bura. The latter quickly devised a programme and "Strela" was put to work. Unfortunately, we proved to have been too optimistic about the elimination of the spurious poles from the amplitudes: the computer showed itself cleverer than man by "skidding" and failing to go through with division by zero.

The mathematicians wondered what might be wrong but we, giving it thought, knew and beat a retreat. Later we learned to indentify all singularities and published a paper, but decided to put off a quantitative solution of the equations until better times, since we had not coped with New Tamm-Dankov Method normalisation. It was a lesson to us all.

Tamm had a thirst for the new. When some interesting article appeared, especially on elementary particle theory, he would be one of the first to study it and would discuss it at his seminars. Schwinger and Feynman's work in the late 1940s, Dyson's work on the *S*-matrix, Lehmann, Zimmerman and Simanzik in 1955, and Snyder's work on space-time quantisation, are but a few examples.

Tamm's attitude to the greats in science is noteworthy. I re-

member well what he told us in 1950 about Heisenberg's work[4] on the *S*-matrix. At the end of the seminar, being young and cocksure, I said something like: "Why on earth should we study that work if the *S*-matrix suggested by Heisenberg fails to agree with causality conditions?" Tamm adopted the attitude typical of him when vexed: hands behind his back and head down, he paced up and down in front of the blackboard and in angry tones delivered a sermon largely aimed at me: "Heisenberg could make a mistake too," he said, "but he is Heisenberg, and before making such categorical statements, you must prove them several times over" — and much more in this vein.[5]

Thus starting out in science, I learned that though the big names in physics were not gods, they deserved study and respect. After the seminar M. S. Rabinovitch preached to me that many people had come to the Theoretical Physics Department with no respect for anyone, but had become more modest with the years, showed more respect and got down to real work. Deep inside I did not agree with my older colleagues, though now I see that they were right.

There was one other occasion, in 1952, that Tamm showed his annoyance. I was 15 minutes late and a real storm broke as I tried to proffer my apologies: he did not let me get a single word in. He rose, inclined his head and showered these words on me: "I am no callow youth to be kept waiting for such a time!"

All my attempts to make an excuse were cut short — no excuses! But his eyes sparkling friendliness gave him away. The storm was over in a minute and his voice changed. "All right, sit down, let's get down to 'nos moutons'," he said, screwing up his eyes mischievously.

As I got to know him better, I could see that he was optimistic and had a vivid sense of humour. These features came through even on the rare occasions when he had to discipline someone. One could not avoid the impression that he might easily have contained his anger where his colleagues and friends were concerned: outward it was accompanied by merry flashes in his eyes. He seemed to force himself to be angry. But to people who were dishonest in science Tamm could be ruthless.

Tamm was always in a hurry. His "free path" was very short, especially at the Institute, and nearly all of the staff knew that. For a popular person like him to enter the Institute and pass to the Theoretical Department or the Conference-Hall without runn-

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[4] Heisenberg W. — *Z. Naturforsch*, 1950, 5a, S. 251, 367.

[5] See B. M. Bolotovsky's recollections of the incident in this book — *Ed.*

ing into people and talking was an impossibility. He used to jot down on a piece of paper all the matters that had to be attended to, and tried to quickly "get the small things out of the way", as he put it. He hurried on, powered by an overriding passion for science, innovation, for the all-consuming and exhausting labour that was for him the only real source of inspiration.

He took a very peculiar attitude to "small things", to the daily treadmill. They were unfortunately unavoidable, he said, but you had not to get bogged down in them; you had to be able to "skip over" small matters. You did not need to answer all your mail like Landsberg who then had no time left for his research work.

I could credit that in the sense that you have to know the extent of your abilities, believe in your powers and, instead of going frittering away energies, quest for major scientific solutions. Yet Tamm would not specify how to go about gauging your abilities. He was fond of all kinds of conundrums, especially at examinations, and he would rejoice should someone arrive at an original and clever solution. Beyond the goal he drew obvious satisfaction from the innovative process. And yet he was never satisfied with his own work. He was always saying he had done very little, had still to accomplish the main thing.

As Tamm got down to work, pondered formulas, articles and his own ideas, or as he discussed scientific problems, he became a different man, oblivious of time. He liked to work at home in his study where no one could disturb him. There he sat amidst papers and books.

Fundamental elementary particle physics aroused his interest the most. He longed to live to see a consistent new theory and disagreed with Dyson who said in 1956 that it would take at least 50 years to do the mathematics. Tamm's pet dream was this: "One day, I'll open *Physical Review* or *JETP*[6] and hit upon the new theory!"

Tamm thought very highly of L. D. Landau, with whom he got on splendidly in spite of their different characters. Yet he would give original pieces of advice. If one of us was to meet Landau, Tamm would come forward with warning tips like these: "Don't pay any attention to Landau's 'general' remarks like 'gibberish!', 'impossible!', and so on. But once Landau proceeds to the specifics, get your wits about you!"

Faraday was one of Tamm's idols. Faraday was a genius,

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[6] American and Soviet leading physical journals — *Physical Review* and *Jurnal experimentalnoi i teoreticheskoi fiziki* (The Journal of Experimental and Theoretical Physics) — *Ed.*

he said, because he could detect links between what seemed totally unrelated phenomena.

Nearly all of our joint deliberations on specific physical problems would give way to more general discussions. He was ever stating that in elementary-particle physics we needed to overhaul our concepts of processes in small space-time volumes. That is why, in the last years of his life, he was so into time-space quantisation.

Tamm was not very keen on writing papers, but when he did, he did it fast, thinking out the logic and formulations beforehand. He drafted all our joint works, and then we would lick them into shape together.

He set much store by select collective discussions of fresh ideas, thoughts or questions. He called it "chewing the rag in a small way", and the ideas just born "Bier Ideen".[7] After "chewing-the-rag sessions" like that, he was fond of repeating Niels Bohr's favourite: "Today I have learned a good deal." He didn't seem to suspect that we benefitted far more. He was a very modest and self-critical man ...

I can still see Tamm as if he were in front of me and I would be happy if what I have written helps the living to preserve his image.

E. L. Feinberg

THE MAN AND HIS TIME

Igor Evgenievich Tamm whose warm handshake is still remembered by many of the contributors and readers of this book was born and grew up in the Russia of the last tsars. It was the time when almost everyone thought their rule unshakable. There were yet no automobiles or cinemas. The gradual replacement of the village taper by the kerosene lamp was a technological revolution. The radio had only just been invented and for many years it was not put to proper use in Russia.

One of the first telephones was installed in the apartment of the Minister of Foreign Affairs to link him with the Gatchina

[7] A phrase current among German physicists in pre-Hitler times, when Tamm used to visit Germany. He was very fond of it. It meant a raw idea born during a chat over a mug of beer — *Ed.*

residence of the Emperor, but the Minister could not manage it and had to call his nephew to help with the new wonder every time he needed to use it.

The Russian Army had just received a modern weapon, the 7.62 mm rifle. People's minds were governed by Tolstoy and Chekhov, Chaikovsky, Darwin and Marx, Roentgen and Curie.

Russia was trying to make up for centuries of feudal backwardness. People were becoming conscious of themselves as citizens and revolutionary ideas were afoot. The Trans-Siberian Railroad was being built and public picture galleries were opening. Intelligentsia was growing up, comprising people of a high technical and scientific knowledge; and the self-sacrificing generations of village doctors and teachers. Some members of the former group had come to play a role of import in government (Vyshnegradsky, for example, was famous not so much as a creator of the theory of automatic control, but as a shrewd Minister of Finance; and Krylov, the mathematician who had translated Newton from Latin, played a major part in the formation of the new navy).

Tamm was born when to many a man scientists seemed eccentrics, and the word "physicist" meant almost nothing. He died when remote control from distances of hundreds of millions of kilometres no longer surprised anyone, when man took his first moon-walk, and Igor Evgenievich could hear a small girl wondering: "There isn't anything so special about a moon-walk. As long as I can remember, people have always been on the Moon." Tamm lived through two World Wars, he saw the time of the grandiose Revolution and the time of fascism. He witnessed concentration camps where millions were incarcerated, and Hiroshima. He died in a country with a social system which differed as much from the Russia of his youth as a 7.62 mm rifle differs from a hydrogen bomb. Millions and millions across the world had become disillusioned with the old ethical values. As a physicist Tamm found himself amongst a category of scientists who were admired, even deified, but also cursed and rejected as representatives of a culture antagonistic to the old model of humanity — a culture which, however, had an appeal to the young.

Can a human being live through such a remodelling of the surrounding world and retain the integrity of his personality? What could become of his character, convictions and outlook, if he drove horse-drawn carriages and lived in a dust-swept provincial town in his youth, and then in his old age could fly from one hemisphere to the other in a matter of hours to discuss what could be done to ward off a nuclear war?

Tamm's credo formed in his young years — his attitude to life, to people, to science, to his own self. His views were not shaken by all the sweeping changes that occurred in the world, by all his personal ups and downs — joys and sorrows, praise and neglect. His firm position had nothing rigid about it. It lay at the basis of never ending development of his spiritual world and life activity.

I was lucky to have known Tamm for nearly four decades; and from him and his relations I heard a lot about his younger years. But it was only recently, when I was given an opportunity to read his letters of 1913—1914 to his future wife Natalia Vasilievna Shuiskaya, that I realised what a conscious, consistent and even tormenting effort he had made to mould his own character. That eighteen-year-old lad knew what kind of man he wished to be. Essentially he had even then been the man I came to know when he was forty.

The main features of his personality contained all the best qualities that the Russian intelligentsia had acquired by the turn of the century. That distinguished social stratum was not homogeneous at all. As mentioned above, it included such varied groups as upper technologists and country doctors. There were people from the underground among them too, to whom Revolution was their life. There were rich, fashionable and often really talented technologists, doctors and lawyers; there were vacillating enemies of the "haves" that would turn now to religion, now to antiquity, to symbolism or futurism, to terror or martyrdom — one could not list all the trends and shades.

The intelligentsia produced poets and revolutionaries, designers and engineers who were convinced that the most essential thing was to build, to create, to do a useful job for the people. Yet there was some basic core, the most important and reliable — a middle-level working intelligentsia with a firm basis of spiritual values. A. P. Chekhov was the epitome of this group of intellectuals, not a "mourner", as some short-sighted critics would have us believe, but the active and immaculately truthful Chekhov, sensitive and subtle, never lachrymose or sentimental, but reserved, even reticent in speaking of his innermost emotions, and merry; the same Chekhov who emerges in his writing and letters, about whom K. Chukovsky has written so well.[1]

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[1] K. I. Chukovsky. *Contemporaries*. Moscow, 1962, p. 704 (in the *Distinguished People* Series) (in Russian).

It is from this background that Tamm came; its hallmarks were his best qualities, its drawbacks his own weak points.

* * *

His inner spiritual independence in big and little things alike, in everyday life or science, must have been his greatest attribute. But he was never pugnacious or truculent, nor contrary, nor protested for the sake of protesting. Nor did he scoff, a thing that is too often mistaken for fortitude. In his letters from Edinburgh the young Tamm wrote much, albeit with juvenile ardour, about his profound conviction that independence should come first:

"All of us reckon too much with the opinions and sentiments of others. As I take it, people are different from the bushes in your garden walk ... only in that the bushes need a gardener to trim them, while man, a thinking being, has learned to do it for himself, that is to clip those sides of his nature which might not tally with the standard. *All people* have sides like that, not only the exceptional. As soon as you have cut off all those twigs and shoots — pooh! your happiness is gone," he wrote, a little embarrassed over his "profundity".

Tamm dreamed of giving himself over to the revolutionary cause. Yet his parents proved a big obstacle. His father, the town engineer in Elizavetgrad (he built the water mains, an electric power station and so on), had a lot of work on his hands, and his mother said that his father's weak heart would not hold out should anything untoward happen. Young Igor fumed in impotent rage, feeling he could not assume such a responsibility. They came to a provisional compromise solution: in 1913 he went abroad for a year's course in Edinburgh, into "voluntary exile" as he put it (farther away from the Revolution). But even there, concealing it from his parents, he became involved with all kinds of socialist groups and Russian emigre political organisations, and studied the situation of the poor.

Igor Tamm had no desire to become a certified engineer because, as he said in one of his letters, "to be a factory engineer certainly means to be on the opposite side from the workers. It may happen that I shall drop politics some day — unfortunately that is possible — but I shall never make truck with the other side and shall never fight against like-minded people."

The Edinburgh "deferment" did not change his intentions,

and when the Revolution broke out, he plunged into it heart and soul.

It was a while later that Tamm drifted away from politics and plunged into science. Small wonder he did not publish his first paper until 1924 when he was 29. In our day he would by then have long since been written off as a theoretical physicist.

His senior friend, Leonid Isaakovitch Mandelstam — an outstanding physicist and profound thinker whom he deeply revered all his life — helped Tamm develop as a scientist. I remember him saying back in the mid-1930s that had Pushkin been alive nowadays he would certainly have been a physicist, and he recited Pushkin's poem about the nature of movement (where the initial line is: "No movement is in Nature, a bearded wizard uttered") and he added, "What a sweeping grasp of the relativity of movement, of the dubiousness of the obvious!" Twenty years later, however, he was saying that we were moving into an era of biophysics and biochemistry and were he young, he would become a biophysicist.

His research soared off in the 1930s. His works on the quantum theory of optical phenomena in solids, on the quantum theory of metals, on the relativistic theory of particles, on nuclear forces, on the Vavilov-Cherenkov radiation, and on cosmic rays appeared in rapid succession, and all of them were significant. They brought him a sense of inner satisfaction, and recognition in the scientific community. But it was exactly in those times that he had tragic experiences both of personal and social character (his brother's death, for one) which cut new deep furrows on his forty-year-old face. He laughed rarely, and profound concentration, coupled with the old vigour of his gestures, marked his appearance for many years.

But his sense of civic duty did not allow him to keep away from acute social issues. In philosophic discussions on new trends in physics, Tamm would advocate the correct understanding, unafraid of grave but unjust charges of idealism. At that time Tamm was particularly vulnerable to attack. However, he never did anything that was contrary to his own idea of ethical behaviour to make things easier and "mend his reputation". He showed the same concern as ever for his relations and friends in distress and, what is most important, remained true to himself and, for all his regard and respect for other people's opinions and prestige, would rather follow his own reasoning.

The independence of his mind found expression in his intrinsic atheism too. In his school years that atheism was manifest

in his somewhat naive and puerile clashes with the Divinity teacher, a priest. As Igor grew up, he attained full inner clarity on the matter. Certainly he never descended to showing any token of disrespect for believers, but he just could not comprehend that anyone could delegate the job of setting norms of behaviour even to some "supreme being" or his representatives on earth. Everyone ought to abide by their own set of moral values. If believers had ideas to offer — fine, he was ready to listen, but he would decide for himself. As it was, he had heard enough in his younger years to draw conclusions.

Or take the Pugwash Movement which emerged in the late 1950s and early 1960s: Tamm was aware that not that much could be expected of it and how much naivity could be injected into it, but nonetheless felt it his duty to take an active part. He hoped that the Movement might bring some, even if small use and, were this so, it must be joined in, no matter who might scoff.

The independence of his mind showed itself in matters widely apart. Just before his seventieth birthday it was suggested that he should be presented with a bas-relief of Einstein. To find out whether he would like the gift, two of his younger friends used a stratagem. They invited him to the sculptor's studio where the bas-relief was. It was done in modern style and many people, including these two friends, liked it. Tamm's tastes, however, had formed much earlier, and though he took Shostakovitch's music seriously and with interest[2] and was all attention while listening to it, he definitely did not like the bas-relief. He was silent and thoughtful, and it was only after leaving the studio that he said gruffly: "No, I really dislike all that." Unlike others, he did not try to copy the younger people's tastes to seem "with it". That independence of thought and action must have played a decisive part in his scientific achievements. It so happened that colleagues of his often took a strongly critical attitude to his work. Here are just two examples.

As the neutron was discovered and it became clear that the atomic nucleus consists of neutrons and protons, this model had to be correlated with the values of nucleus magnetic moments. Earlier Bacher, an experimental physicist, had observed that the magnetic moments of nuclei could be deduced if one ascribed the magnetic moment (the negative one) to the neutron itself. Tamm working with one of his postgraduate

[2] See V. D. Konen's reminiscences in this collection — *Ed.*

students, S. A. Altschuler, analysed the data available and came to the same conclusion.[3] Now that we have got used to the picture of spatially extended hadrons with their complex arrangement of electric charges and currents, it is hard to believe that at first this was cast aside as absurd, as heresy, pardonable from some experimental physicist, but not from an educated theoretician. It was then regarded as certain (and as the only concept consistent with the theory of relativity) that elementary particles were point-like, and that the neutron, which did not carry on the whole an electric charge, could not have magnetic moment. Tamm presented his paper at Kharkov in 1934, at a conference which brought together many big-name physicists from this country and abroad. Tamm told me how those people, who felt affection and admiration for him and whom he in his turn respected, even with some compassion, tried to explain the absurdity of his conclusion to him in a whole variety of languages. He listened to them attentively, argued passionately and stuck to his guns — he could see no cogent refutation. Later, and not that much later at that, it became clear that he was right.

Twenty years later, as pion physics started making progress at accelerators with an energy about few hundreds of megaelectronvolts, Fermi found that pion-proton scattering was a resonance scattering. Tamm received the Fermi results as proof of the existence of short-lived, unstable particles. That idea caught his imagination, and his enthusiasm spread to a group of young research workers. Tamm launched large-scale investigations into various pion processes on the basis of a single idea about the existence of resonances in the nucleon-pion system; good results were obtained for pion-nucleon scattering, for pion photoproduction on nucleons, and for proton-neutron interaction. Tamm called nucleon-pion resonances "isobars". Tamm's Theoretical Physics Department was in full swing with "isobars", the order of the day. The late Semyon Belenky (who successfully applied "isobars" to the Fermi statistical theory of multiple production) wrote a series of rhymes in which every other line ended on "isobars":

Ding dong derrie dahs
Let's buckle down to isobars.
Come to us from near and far,
Come and work with us on isobar.

[3] See S. A. Altschuler's article — *Ed.*

It became clear at the very outset that a satisfactory quantitative description of these processes could only be obtained by postulating a large resonance width, over a hundred megaelectronvolts, i. e. a little less than the level value. That suggestion attracted scepticism from some of the best theoreticians outside the Lebedev Institute (though they were personally very friendly towards Tamm). Indeed, could one speak of such resonances as real? The point is that the approximate method was used for calculations taking no account of multipion states — after all, how could one trust the numbers! An enormous calculating job was done nonetheless. And Tamm could see that he was on the right track: he saw that the results were stable against changes in the approximations. No matter how outspoken and authoritative his critics might be, Tamm would not yield; he persisted in defending the reality of resonances. And in a short time resonances became full-fledged members of the family of elementary particles.

Yet it would be utterly wrong to maintain that Tamm was deaf to criticism. His strong point was that he always took his opponent's arguments seriously and was prepared to admit being wrong. How many times did he refute his conclusions and how many times, having told our private "Friday" Department gatherings about his results, would he a week later repent, bitterly and unreservedly. Pacing up and down in front of the blackboard, he would speak hurriedly and say that he was sorry that "last time I talked a lot of nonsense", that he was ashamed, and so on. Sometimes that happened after one of the younger fellows pointed out the mistake.

But before publication, he would check and double check to be quite certain. I cannot recall a single published work of his that was erroneous. There might be one or two publications with rather particular hypotheses of which he was himself uncertain, but he published in the hope of sounding the experimental physicists, and which were proved unjustified.

He remembered his mistakes better than some of his older works which had ceased to interest him: he remembered them and did not try to hide them. Early in the 1960s he told me about a political dispute with Niels Bohr. It was in 1934 as they were returning by train from Kharkov to Moscow. Those were troubled times, with Hitler menacing the world. Bohr insisted it would be possible to restrain him if all the anti-fascist forces — Communists, Social-Democrats and Liberals — joined hands. "I wonder that you don't understand that, Tamm. It is imperative!"

As a matter of fact, what Bohr was talking about soon came

into being — the Popular Front in France, the Spanish Civil War and, later, the resistance movement. But Tamm adhered to the then current view that such an alliance would weaken the anti-Hitler struggle. They argued all night long, standing beside the window in the corridor. How he lamented his short-sightedness (was he alone in that!).

Tamm did not have to "squeeze the slave out of himself" like Chekhov, who grew up amongst dull shop-keepers and philistines. He could make mistakes, he could put too much trust in the conventional words that, with time, change their meaning; but even obeying necessity, he was never a slave.

* * *

Tamm was a courageous man, and he was also brave in the conventional sense. He remained cool and level-headed under shelling at the front during World War I. In a letter to his fiancee of May 23, 1915 he described one such bombardment, writing with satisfaction: "It is ghastly indeed as, standing in the open, you hear the ominous whistle. But you somehow hold yourself together." In the Civil War, while wandering between the Crimea, Elizavetgrad and Odessa, he landed plump in a mess of "governments" of every stripe, bandits like Makhno among them. Tamm had been in mortally dangerous situations where only his self-possession saved him. Incidentally, he was a veteran mountain-climber. Despite the dangers, he would go to the mountains again and again.

One episode is indicative of his idea of fortitude. His son was an even higher class mountain-climber, a Master of Sport, and he led parties on unique and very dangerous climbs. I never heard Tamm praise his son, of whom he was very fond; on no occasion would he permit himself to "boast".[4] Or rather, speaking so

[4] Self-praise was totally out of character with Igor Evgenievich, except in jest, e. g. when playing volleyball. I never heard him at any scientific meeting inserting comments like "I said that earlier" or more modest versions thereof. Hate of boastfulness was a feature of himself and his whole family.
Once his grandson returned from kindergarten and Tamm asked what he had done there. "We weaved baskets", he answered and added uncautiously: "Mine was the best." The burst of laughter fixed the expression forever. Many years have passed and Igor Evgenievich is no longer with us; his grandson is a grown man with a family of his own, but the phrase "my basket was the best" stays in the family as a symbol of boasting.

matter-of-factly about the family news, he would patter off something like this: "Zhenya is back from a winter traverse, class 5b" (top degree of difficulty). I can only remember one occasion when he could not contain himself. A group of his son's expedition ascended Mount Khan-Tengri (22,750 feet), while he himself, following in the next line, was held over by foul weather for two days just a few hundred yards from the goal; taking a sober view of the situation, he ordered a retreat. Tamm was full of admiration. "Good lad! It must have taken a lot of courage to take the responsibility and deprive himself and his comrades of the joy of victory when a stone's throw away!"

Courage based on high intellect: that was Tamm's remarkable feature, especially in the last years of his life when he was struck down with an incurable illness. All his life he had enjoyed good health, had never been seriously ill. He was over sixty when one Sunday evening he told me jubilantly: "Yesterday I learned that one can gain a second on a hundred-metre run. All you have to do is to run those hundred metres beforehand." And this lively man who always walked as if seeking to arrive before himself, was rushed to hospital because of degeneration of the nerve that controls the diaphragm. He was operated on and put on a breathing machine: a metal tube was inserted into the trachea and hooked to the respiratory device which blew the air into the lungs at an even pace, in the rhythm of natural breathing.

I had anticipated that moment with horror, almost sure that his courage would make him tear that pipe out, putting an end to this miserable existence. But I had a simplistic idea of his fortitude. A few days later, they let me visit him at the clinic. It was the first day he was allowed to sit in an arm-chair for an hour. He still was not used to speaking under the new conditions — he had to utter words in accordance with the machine breathing. I had only been with him a minute or two when Professor Lyubov Popova, the head of the respiratory department (she had supervised the operation and guided the treatment until the very end), took me to her study and asked me with anxiety: "Have you seen what Mr. Tamm wrote today? Sitting in the arm-chair, he motioned for a drawer to be taken out of the table; he turned it upside down, put a sheet of paper on top, and started writing mathematical symbols. Have you seen them? Are they all right?" What she meant was: "Is he all right in the head?"

It seems I failed to convince her that he was merely going ahead with his work, continuing the calculations interrupted by going to hospital. Lying still after the operation, he must have thought something up and was impatient to check it out. "But this is impossible. Everyone who undergoes this operation suffers

a psychological trauma and goes to pieces.[5] It takes quite a while to recover," the doctor objected.

But Igor Tamm did not "go to pieces" — work was his remedy. For three years he was on respiratory machines. He had two at home — the "big one" beside his bed, the other one at the desk. He would get up, shift to the table and work for several hours on end hooking himself to the other machine. The mechanical link of a living person to a snorting machine gave everyone a shock the first time they saw it. But Tamm was not broken by it. Caught up by some idea on particle theory, which might just resolve the fundamental snags in the theory (he would curse himself for being unable to leave it off before finding out for certain whether it was right or not), he went ahead with his calculations covering the pages with four-figure numbers.

In 1968 the Academy of Sciences awarded Tamm the Lomonosov Medal. Under the rules, a winner is supposed to report on his findings to the Academy at the General Assembly. And Tamm, chained to his apparatus, decided to stick to this tradition as far as it was possible. He wrote a twenty-five page paper remarkable in that it dealt not only with past work as is customary but also with his current work, his plans and forecasts for particle theory. The paper, read out at the meeting, contained his typical optimism. Its final phrase was: "I hope that we shall live to see a new stage of the theory, whatever it might be."

As people took their seats in the hall, Academician Dmitry Skobeltsyn (who had earlier wondered whether Tamm's paper would be read) said to me with regret: "It will be a mere formality, of course. The paper has been written for him." But when the reading was over, he said passing by: "Well, it's quite clear that Tamm wrote that paper. That's his all right, every bit of it."

* * *

Tamm was an active man. Trite phrases like "we only live once", phrases meant to justify a consumerist mentality of the vulgar, had a different dimension for him. They meant a more stringent requirement to him, endeavour to achieve the maximum: to leave a mark on science, help the people, and achieve to the best of his ability, however much or little that might give humanity. In his young years a frequent complaint in his letters to Na-

[5] As a matter of fact, his was an exceptional but not the only case of such psychological stability. If equanimity comes, it comes much later.

talia Vasilievna was about wasting time, about forced inaction for some reason.

He had an intrinsic sense of involvement with humanity. This explained, among other things, his passion for politics in his youth, his fight against pseudo-science, and his part in harnessing atomic energy to ensure an equilibrium in the world, an essential condition, as he and many others believed, for averting nuclear catastrophe. But it often happens that while showing concern for all of humanity, one neglects an individual. But let me tell one thing. In 1953 honours and awards were showered on Tamm. He had seen nothing like it in all his life. One day he took me to his study and said: "I have received a very large sum of money as a prize. I don't need this money at all. Do you happen to know some young people who might need assistance to carry on scientific pursuits?" Recently I discovered that he put this question to others too, and he acted with promptitude in all cases.[6]

Here is the usual picture: with a talk coming to an end in his study at the Institute, he would suddenly pull out a cigarette packet or envelope covered with notes that only he could decipher, and (with a cigarette in the corner of his mouth and screwing up his eyes against the smoke) say: "Oh yes, I've got to ring up this fellow, put in a word for this one and learn about that one" etc.

His active nature kept him working all the time. He liked to work at night; the bulk of the results he achieved were rarely published, for he only sent really substantive things to the journals, and the number of papers he published is incredibly small; by the present standards there will be 55 papers at most, if we don't count popular science articles, reviews and reprints in other languages. Sometimes, after a failure to solve a time- and energy-consuming major problem, disappointment would come; and if he had no new idea about it, he would feel dejected and unhappy. At those times he would come to the Institute and say to younger colleagues: "Come on, toss up some little problem." He would call it "taking a hair off the dog."

Those "hangovers" gave birth to unexpected works on rather specific problems — relating the theory of elasticity (with L. M. Brekhovskikh), oscillations of an elastic plate in contact with water, a work done jointly with V. L. Ginzburg on the electrodynamic theory of the laminated core. Tamm worked in a wide range of areas, and having a good professional grasp he could easily achieve results in the most diverse fields of physics.

However, the work on the laminated core merits special com-

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[6] See S. M. Raisky's contribution to this book — *Ed.*

ments. It dates back to the wartime years and the situation had a deep meaning. With the Institute evacuated to Kazan, Tamm stayed there from August 1941 to September 1943, and was very unhappy. In those trying times he took it hard to be away from the most vital problems facing his country.[7] He took part in calculating complex configuration magnetic fields, helping A. P. Alexandrov and I. V. Kurchatov with their important job of protecting ships against magnetic mines, and he was glad to have a real job. Then he calculated an optical system for spectral devices that the defence industry urgently needed,[8] and took part in a study of properties of explosives.[9] Yet all that was too simple and it did not satisfy him. He felt that his talent and experience were being wasted. I had never seen him so irritated, so put out. It seemed that being used to making do with little, ascetically unpretentious in his daily needs, Tamm found the need to provide for the daily bread of his family in the rigors of the hungry hinterland humiliating. Against the backdrop of the country in danger, it was an excruciating experience.

Earlier, when World War I broke out, he received it "antipatriotically", as they said in those times. He understood that it was an "alien" war, and at first did not want to volunteer for the front like other students (as a Moscow University student he was exempt in the initial war years). His wife Natalia Vasilievna recalls how furiously he would argue with a "defensist" member of their family. But the bloodshed continued on the fronts, and in the spring of 1915 he volunteered as an orderly in a field hospital. He saw torrents of blood and handled hundreds and thousands of wounded. He saw suffering that he could never forget, and felt a need to understand, to fathom out what had caused it — he could not do otherwise. In World War II things were different. He suffered deeply because he felt he was not doing enough for the common cause. Naturally, he continued working intensely all that time — Tamm just could not live without science. His topics were elementary particles, nuclear forces and similar problems which, in the early war years, seemed very remote from applications. Few people could foresee that a few years later those "abstract" and "untopical" problems would be some of the most vital. And Tamm took it hard.

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[7] With bitter humour, he recalled Napoleon's order during the Egyptian campaign as Mamelukes attacked: "Form into a square, donkeys and savants in the middle." Donkeys were the precious transport, and savants accompanied the expedition.

[8] See S. M. Raisky's article — *Ed.*

[9] See V. A. Krainin's contribution — *Ed.*

We will have to resort to still another worn expression in saying that Igor Evgenievich's conduct was in strict agreement with his firm principles. If we exclude from these words a certain tint of pompousness of speeches at jubilees we shall see that such was really the case. Of course much of what has been said above testifies to this, but I would like to lay special emphasis on the way Igor Evgenievich argued in defence of science, and fought pseudo-science. Unfortunately many consider the art of polemics to be in dazzling one's opponent with high-flown formulas and expressions, and sharp attacks even to the extent of demeaning the other party.

Igor Evgenievich would have nothing to do with anything like that. Though an eloquent, involved teller of his own picturesque adventures he or anyone else met when travelling, or funny, sad or tragi-comic stories, he was short and to the point in public debates and speeches. He was always seeking the truth and to convince an opponent solely with reasoning and knowledge. He wanted to bring the other fellow to his own truth too. Personal elements were just not him. Being truthful and honourable himself, he assumed the same of others, which of course was not always the case. Here are three examples that come to mind.

Speaking at a session

Strange though it may seem (this phrase I find myself repeating but much that happened in Tamm's lifetime is hard to believe today), even in the thirties there were accomplished scientists who considered the electromagnetic field the result of mechanical motion in the ether. The most active proponents of such a view were Professors A. K. Timiryazev and N. P. Kasterin and electrotechnologist V. F. Mitkevich, a Member of the Academy. The problem was that they asserted that dialectic materialism supported them and people outside physics believed them.

Igor Evgenievich, being the man he was, could not remain on the sidelines. He was the author of a course on the electromagnetic field for Moscow University, and in his youth had studied Marxism and Marxist theory. He offset the biting, demagogical remarks of those gentlemen with serious arguments and no more.

He found a fine example to show the conventionality of the lines of force concept and the fictitiousness of the lines number concept. Here was his argument. Where there are two electric currents — linear encircled by circular — the resulting magnetic field is of toroidal shape: the magnetic line of force snakes over a toroidal surface around the linear current. If the ratio of strengths of the two currents is a rational number, a line of force will eventually close on itself. But one only has to make the slightest alteration to one of the currents for the ratio to become irrational whereupon the line of force will never close on itself. In that case an infinite number of lines will pass through any cross-section of the tor to form a blanket toroidal magnetic surface.

It might seem that any physicist should comprehend that numerical density of lines of force can only be a conditional measure of electric field strength, and a line taken in isolation cannot be real mechanical movement in the ether. I should add that this example (contained in the first edition of Tamm's course in 1929) was to find a practical application in the seventies — such magnetic surfaces are of major importance in stellarators (one of the fast developing versions of controlled thermonuclear fusion).

My second example dates to 1936. There was a session of the Academy in the huge auditorium of the Communist Academy at Volkhonka Street. A. F. Ioffe reported on the progress of his Leningrad Physical-Technical Institute. Long debates developed into general organisational and physical issues, and the young Landau and Leipunsky spoke out against Ioffe, accusing him of undue optimism. I don't remember when exactly Igor Evgenievich entered the debate, but he objected to V. F. Mitkevich who again supported the mechanistic theory of electromagnetism.

As ever Tamm was short, sharp and to the point. The hall was keyed up by what had gone before. There was a big attendance and many, including myself, had to sit on the steps between the benches. Igor Evgenievich set out to display the inappropriateness of certain mechanistic notions to electromagnetism. His opponents demanded to know what substance moves in the space between two electrical charges when one of them is displaced.[10]

Tamm said: "There are questions for which no intelligent answer can be given; for instance, what colour is the meridian which passes through Pulkovo? Red or Green?"

Academician Mitkevich burst in with:

"Professor Tamm does not know the colour of the meridian on which he is standing, but I know I am on a red meridian."

Igor Evgenievich merely looked in surprise at the speaker, shrugged and ceased to continue.

And my final example. In the mid-50s Tamm joined with biologists, physicists and mathematicians to fight vigorously for genetics research which had come to the fore worldwide. In October 1956 a General Assembly of the Academy was convened to re-elect the President, A. N. Nesmeyanov. The matter would have seemed a mere formality since the Members had a high opinion of the man as a scientist.

Tamm was on the best of terms with Nesmeyanov. But when he took the floor to pronounce a long speech in his calm and convincing manner, he added to favourable comment on the President, including his confidence in the man being of progressive outlook, sharp criticism, especially on what he considered insufficient activity for the development of biology. Tamm proposed that the President's re-election be postponed until the latter had reported to the General Assembly with a clear plan of measures to be taken. Repeating many times his respect for the President, he said that such a decision would help him since the support of the General Assembly would give him greater prestige and vigour in his difficult work.

Igor Evgenievich had earlier made this proposal at a meeting of the Section of Physical and Mathematical Sciences which had endorsed it. All the other Sections, however, were for election without conditions. Tamm's speech caused uproar. It made such an impression that a compromise was decided on: A. N. Nes-

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[10] These opponents asserted that mechanical movement of substance was an essential primary element of any physical processes interpreted materialistically. In reality a change in the electromagnetic field is accompanied by movement of energy stored in vacuum even without any bodies being present.

meyanov would be re-elected, and an Extraordinary General Assembly would be called in the near future to hear and discuss his report. This meeting took place in December with about thirty Members taking part in discussion. For the first time in many years the Academy engaged in broad, blunt and frequently even bruising debate.

Igor Evgenievich in general could not tolerate pseudo-science. At the end of the forties certain authors renewed the assault on the theory of relativity and quantum mechanics, calling them "bourgeois" and "idealistic". Some professors tried to adapt the theories to make them more palatable for the critics, this even at the price of vulgarisation.

Of course, contemporary science had strong enough roots by that time in our country. The defence by big names such as I. V. Kurchatov, V. A. Fock and S. I. Vavilov did its bit. And there were plenty of rank-and-file scientists who were not going to be intimidated. But the position was far from simple. Tamm would have none of the apostasy that is born of career-mindedness or timidness. He would not forgive even those who had been close to him, breaking off personal contacts with them.

* * *

Tamm presented a happy combination of good-natured vivacity and mirth, candor and sociability, with restraint, profound concentration, tact and propriety. At the same time, he could act on impulse, could be short-tempered and irritable. Mirth, vivacity and impulsiveness were for social intercourse, for recreation, for lectures, especially popular science; short temper and irritability applied to things secondary or paltry, to the daily bustle that got in the way of work and life. But when it came to anything essential, serious or significant, we faced a different man: only well-considered words, cogent arguments, a sense of justice in attitude and pronouncement; no haste, nothing vague or extraneous.

Tamm could be quite gregarious in company; he used to talk well and to savour everything he had to say. He was often in the focus of attention, was always ready to throw himself into any prank, trick, charade, game or jovial contest, and he would rejoice if he won, and was furious with himself if he lost. But even here, amid noise and hilarity, his tact, both inborn and acquired, would never fail him: he would never stand in anyone's light, would listen readily to other people; and if he put in a word or two, it was timed so as not to interfere but to help the speaker and listeners.

Alas, such cultured behaviour is not a feature of many in the later generations. One day a prominent person came to him for the express purpose of making his acquaintance; Tamm took an interest in him. He had also invited two of his younger friends. Very soon, interrupting each other impatiently, they turned all the talk to themselves alone, and Tamm could do nothing but hum and haw, smile and with a twinkle into his eye put in the odd word here and there. So no real conversation with the guest came about. Yet he never reproached his young friends. Moreover, it turned out that he himself took no notice of being so unceremoniously pushed aside.

For all his communicative nature, Tamm was chary of expressing his feelings. I think that you can judge about a man's inner character by the way he plays chess (it used to be cards a century or two ago). The emotional and intellectual tenor of a physicist engrossed in his calculations (and maybe of scientists in general) appears to me similar to what one feels over the chessboard. You have to break the "enemy's" resistance, and see many moves ahead, forestall difficulties and mishaps; you've got to take stock of the weak points in your position and all possible developments, while sticking to an overall plan and aim. You must be careful not to make a silly error in your calculations; you have to hold quite a lot in your head and keep moving, processing all the fresh information as you go. It is another thing with the player's reaction to success or failure, to his opponent's behaviour etc.

Chess was one of Tamm's most ardent avocations. He was a mediocre chess-player with a low rating. Yet his style was revealing in a variety of ways. First of all, he could switch instantly from a casual or animated conversation to total concentration once the first move was made. His singlemindedness was overpowering. If someone, his opponent or an onlooker, made a joke, Tamm would take no notice or, at best, twist his lips into a momentary smile.

He made his best moves in hazardous situations. On occasion there seemed to be no way out, yet the long tense concentration and the passionate desire to hold his ground or win could yield quite unexpected results. Having made a good move in a situation like that, he would shift his cigarette into the other corner of the mouth, press his hands between his knees and look up at his opponent and down at the chessboard every now and then, all tense with expectation; or he would fumble for his cigarettes and matches which he always seemed to have mislaid. He took defeat badly. But as in everything, he never gave vent to his feelings, though this took great effort from him. Here

his striving for selfassertion, which played a notable part in his life, was wounded. His attitude to chess was akin to his behaviour in his scientific work.

As I said before, Tamm never poured out his woes to other people. He had commiseration for others, but expressed it in a very reserved manner. It was in his last years only, the years of his illness, that his sentiment and mildness came into the open.

Years went by, with all their ebbs and flows, joys and sorrows, great misfortunes even; but deep within, Tamm remained essentially the same. His natural qualities, his mental alertness, his interest in the world, his dedication to science, goodwill and high principle were the same. Yet trials and sorrows left their mark on his face, that same intelligent and mobile face.

* * *

Tamm had a deep sense of dignity. I would go so far as to say he was a proud man. But I think I must explain my use of this word. It was not the pride that the vulgar equate with hubris. The Russian intelligentsia, from whose midst Tamm came, had their own yardstick. Seeing Tamm going out of his way to meet someone he liked, his sheer joy, his fuss in "wooing" him, showering words and drifting into emotional speeches, be it Niels Bohr or some obscure mountain-climbing friends, a dull and simple-minded onlooker might even take pity on him for lack of dignity, something frequently associated with august manners and pose. That misled some people. A churlish jack-in-office who, sprawled out on the sofa, talked up to a standing Tamm, a Tamm at first so polite ("ingratiating", the ill-bred bureaucrat might think), would leap in fear as that urbane man suddenly blew up. Insiders can remember other times when Tamm lost his temper for some less conspicuous cause.

But I cannot remember more than three or four such occasions in many decades as Tamm, confronted with lack of respect or downright boorishness, let his temper run away with him. Both we the witnesses and Tamm himself would feel awkward. I am quite positive (and I have good grounds for this) that at such moments he was probably worried up over other, more important things, which he would not tell people (as the saying goes, "shouting at the cat, but meaning the daughter-in-law").

Had Tamm heard the word "pride" associated with himself, he would have been surprised, have laughed or been indignant, who knows. He ridiculed such high words. But what should we call this sense of independence, this adamant stance? What

should we call his calm reaction both to the long official neglect and later to official rewards?

In 1933 Tamm was elected a Corresponding Member of the Academy. He had done perhaps his most significant works by the mid-1930s: a theory of light scattering in crystals, including combinational (Raman) scattering being the first to consistently quantise lattice oscillation; a consistent secondary quantised theory of light scattering off electrons which proved, among other things, the indispensability of negative energy levels in the Dirac theory, a discovery of cardinal significance; calculation of the positron lifetime in a medium; a theoretical prediction of electron surface levels in a crystal called "Tamm-levels"; a fundamental work on photo-effect in metals; and finally, a theory of beta forces between nucleons. In 1937, with I. M. Frank, he explained the Vavilov-Cherenkov effect and evolved a complete theory of it.

The time between 1930 and 1937 was a period of staggering creativity when Tamm's potential revealed itself impressively. All the physicists regarded him as one of the leading theoreticians. Ehrenfest, who intended to leave his academic chair in Leiden (which he had taken over from Lorentz), pointed to Tamm as his most suitable successor. Fermi thought very highly of Tamm's work on beta forces (1934) and saw Tamm as a major theoretical researcher (as testified by Pontecorvo, who worked with Fermi at the time). But the Academy did not elect Tamm a full member. That happened only 20 years later in 1953. Yet no one ever heard him complain or express bitterness about that or take it as offence. Such negative responces in others surprised him. It was different though with unsuccessful attempts at evolving a complete theory of nuclear forces. That sort of thing worried him far more, even upset him.

But here is an inverse situation. In 1958 Tamm, I. M. Frank and P. A. Cherenkov won a Nobel Prize. Since then our scientists have received several Nobel Prizes; but at that time such top-level international recognition of our scientific achievements was a sensation. Only one Soviet researcher, N. N. Semyonov, had been awarded the Prize before. To my knowledge, the award came as a complete surprise to Tamm. On hearing the Nobel Committee's decision, I rushed to Tamm's study and offered him my hearty congratulations. Hands behind back, he paced the room quietly, if anything slower than usual, and replied gravely:

"Yes, of course, it is pleasant indeed; I am pleased... very pleased... But there is just one thing to it."

It was an easy guess:

"That the Prize was not awarded for the work which you consider your best, the beta forces."

Yet the supreme manifestation of his sense of dignity or pride, whichever one calls it, was that he would always choose what he believed were currently the key areas for research, no matter how difficult. I do not know whether this was a conscious principle or whether it was just part of his nature to seek to do the impossible, to outjump himself. Could he have brought himself to abandon that principle, with his ability and erudition, his brilliant professionalism, industry, immaculate calculations and technique, he could easily have produced far more good results.[11] This is evident from his works on the width of a shock wave front, the magnetic confinement of plasma in controlled nuclear fusion etc. But he would not have been satisfied by such works. Small wonder that the natural decrease in his scientific potential with age was a tragedy to him.

In the early 1960s he hit upon a new grand idea — to base the elementary particle theory on the concept of non-locality with non-commuting operators of coordinates of four-dimensional space (and of four-dimensional momentum) and including elementary length. The theory was to be constructed in the momentum space of variable curvature. He reported on his first general considerations and first attempts at an international conference in Dubna in 1964 and at a conference in Japan in 1965 organised to mark the anniversary of the Yukawa meson theory (he was seventy that year).

Realisation of that idea was immensely difficult both mathematically and on the theoretical physics plane. With his excellent command of the essential mathematics, Tamm went on a "working spree", as he put it. He kept up that work in hospital and at home to the last months of his life. Many theoretical researchers were sceptical about it, but he had learned to live with that. He did not complete his work. So it is not yet known whether this "mad idea", one of the many researched by theoreticians all over the world, will yield anything.

The same refined sense of dignity marked Tamm's attitude to the delicate matter of priority in science. A scientist must have some ambition. The important thing is how it expresses itself and how it influences his attitude to others. It seems to me that in Tamm ambition was only self-affirmation and, more significantly, self-affirmation in his own eyes. He did not aspire

[11] While writing these lines, I did not know L. D. Landau's pronouncement cited by S. A. Altschuler (see page 29 of the present collection).

to rising in the estimation of others or earning their admiration. He wanted to know deep down that he could do it. He derived inner satisfaction from reaching a formidable objective: testimonies of recognition were no more than a pleasant extra. I do not know of a single time when he reproached anyone, mildly even, for using one of his ideas or for failing to refer to his work where they should have. Unfortunately many others do not feel that way. This issue has for some people become a disease.

I would like to recount one episode to illustrate Tamm's attitude to priority. In the early 1930s an idea occurred to him from which he produced an excellent work that greatly advanced the theory on the subject. He did the job — extensive and very involved calculations — during a conference, working at night as usual. When he had finished it, it turned out that the final formula did not live up to expectations as a quantitative description of one essential phenomenon. Be that as it may, the work was of some consequence, and Tamm prepared a short paper.

At this point a young theoretician, who used to visit him each morning at his hotel, approached him asking whether he would object to his sending a letter to the journal: "After all, we discussed the matter together many times." Tamm was surprised, but would not say no. The two items were published simultaneously — Tamm's paper containing, in addition to an explicit physical formulation of the idea, the final formula and the negative deduction from it; and next to it the young researcher's letter to the editorial board dealing with generalities, the "idea". However, that letter gave him dubious ground for claims that his name should be placed alongside Tamm's as a co-author of the whole theory.

Tamm told me the story a quarter of a century later, chuckling and with no bad feelings. When I had finished these reminiscences, I learned that Tamm had mentioned the incident to two of his closest associates. I write of it not to denigrate or injure anyone, but only because this episode is eloquent of Tamm's attitude to priority. It was important for him to know that he *could* do the job, and if someone else wanted to share the outward recognition without having done anything, then let him, it was only funny. I feel this explains the sense in which one can call Tamm a proud man.

* * *

How Tamm got on with his pupils is a special matter. He had a large following and his long teaching, lecturing at Moscow

University, at the Moscow Institute of Physics and Engineering, and then again at Moscow University, a course in electromagnetic field theory, had a great influence on whole generations of physicists. It may seem a paradox, but he had no well-thought-out system of teaching. The brilliant school of theoreticians formed by Landau emerged from his careful plan for drawing raw beginners into science: at first examinations on the famous "Landau theoretical minimum", carefully thought-out and compiled, then synopses of literature at the seminars, and finally research work. This system incontestably produced splendid results. Yet another approach is also possible.

Tamm read lectures that were of interest to him. He did not like to repeat the same course several times, and it is easy to understand why. I heard him first in 1932 at Moscow University, when he was lecturing on electromagnetic field theory. He had read that course many a time, the second edition of his *Fundamentals of the Theory of Electricity* had been published and, as he confided to me later, he was fed up: "I know my book as well as a learned Jew knows his Talmud: if you stuck a pin in the book, I'd know the exact word it went through on each page."

Nevertheless, he would light up at his lectures and spark the students as well. They loved his lectures. No doubt, his personal charisma played no small part.

He usually began to work with his students only after they graduated, sometimes even several years later. I can recall only a few cases (three or perhaps four?) when he took on new graduates. As to himself, in his varsity days he had selected disciplines according to his taste. The teaching system at Moscow University, and even more so at the University of Edinburgh, gave him considerable freedom of choice. As one can read in his Edinburgh letters of that time, he saw immediately that by and large he knew the first two years' physics courses, so he stopped going to the lectures. He signed up for second year mathematics right away, and learned a remarkable lot. He also took chemistry and several languages, studied philosophy and *Das Kapital*. After graduating from Moscow University and before he came into contact with Mandelstam, he would study on his own. He must have hoped to find the same independence in his students, notwithstanding the rigidly regulated curricula as common in our country. When someone told him that one of his students was greatly enamoured with him as early as the third year, Tamm showed surprise: "I just cannot work with the students, I don't give them anything."

It was chiefly his example, his attitude to science that worked — and not mere logic of reasoning, choice of examples for

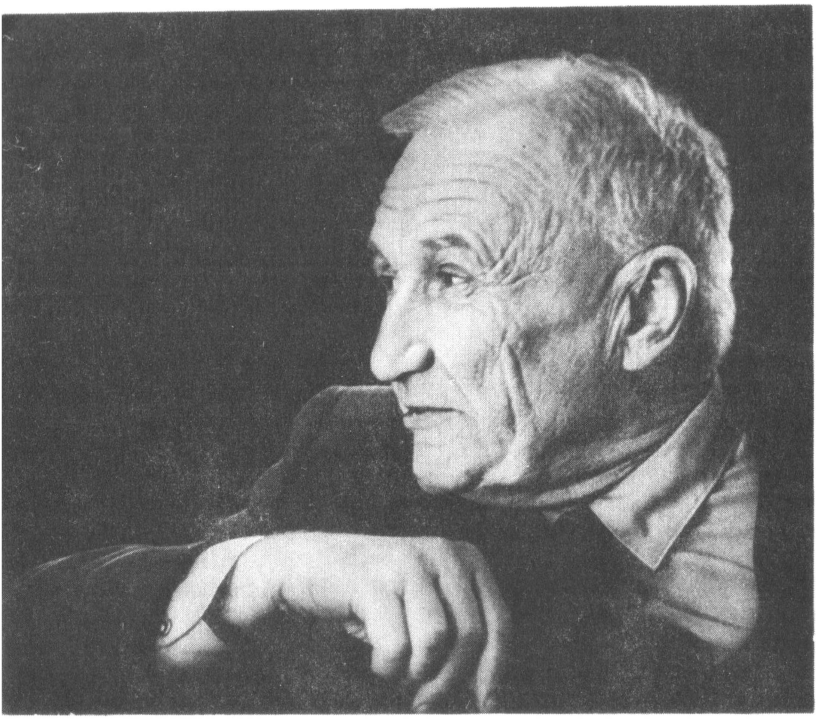
consideration, or the measure in which physics was combined with mathematics, and the conventional material in the manuals with new topical questions. It was more his involvement as a lecturer, his vigour, the obvious pleasure he derived from finding the path to truth. However, things did not go well with the graduates that I remember. He did not find a rapport with one of them, whose graduation paper he had liked, and so they drifted apart. He did not want to take on a second, though that person was to become a good theorist.

With would-be students it all began like this. They would bring in questions, results and ideas which might reveal that all-important independence of approach, and an ability to work. That always struck a responsive chord in Tamm. In physics terminology, one might say that his "response function" was stepwise with very big steps. Tamm would press hard for such a hopeful's admission to a postgraduate course if there were any difficulties in his way, or for his release from a factory laboratory if the management there insisted on keeping him, and so on.

Even after a young man began to work with Tamm, there was no room for uniformity of methods. He took it for granted that he was dealing with an independent mind, a colleague who only needed to draw on his greater experience. Tamm, even as a renowned scientist, retained the custom of his youth of addressing everyone by name and patronymic, students included. I can recall one man only whom, after 25 years of work in the Theoretical Department, he would *sometimes*, in rapid speech, call by his first name alone. It is unimaginable that he could use a familiar form of address to anyone, even the youngest, as is often done nowadays.

That must have been an essential psychological element in rearing a thinking research worker, in dispelling that authoritarian atmosphere that can lead to a postgraduate hanging on his tutor's every word (I know this does not always have such an effect, and I could think of several examples when casual familiarity and neglect of the patronymic did not affect a postgraduate's independent outlook, and I can also recall cases where addressing a postgraduate by his first name and patronymic did not help the tutor's prestige). V. L. Ginzburg and I worked with Tamm for 30-35 years, from our postgraduate days. It was only in his last years that he insisted that we should use first names. But we could not call him "Gora" as his relatives and childhood friends did. We only agreed to an asymmetrical version thus turning down his suggestion.

But let us return to Tamm's cooperation with his students. Some were drawn into joint work on a subject he suggested;



I. E. Tamm (1965)

others were "offered" a subject and struck out by themselves, only occasionally asking him for advice; still others would choose a subject to their liking, sometimes outside Tamm's range of interests, and would come to discuss specific stages of the work or the final results, to hear his advice and provisoes.

So what was his magic as a teacher, and how can one speak of him having a "school"? The most important elements here were Tamm's geniality and attentiveness, helpfulness, and a frank assessment; the example of his own assiduous application and vast erudition; an ability to combine a physical approach and physical understanding with a coherent mathematical interpretation; broad use of similar elements in the various divisions of physics with accentuation of the most vital areas in each division; his fostering of an attitude to other people's works which combined respect for a renowned author (and for the tutor himself) with sharp critique, and caution where a new,

hitherto unknown name was concerned, with an in-depth analysis of the work — an analysis, allowing for the possibility of appearance of a new talent; and lastly, an atmosphere in which work on "applied" themes, drawing upon both good physics and high professional skills, would be as much appreciated as research into "high" physical problems.

Here is an example. N. came to Tamm as a postgraduate and worked with him for several dozen years. In all that time they did not publish a single joint research article. After initial unsuccessful attempts at joint work, Tamm just once suggested a research theme for N.'s thesis. N. performed the research and showed it to Tamm just when it was ready. Then N. worked in areas that did not interest his supervisor (so it was impossible to consult him) and partly in those which were closer to him but which he had never dealt with. But N. lived in the atmosphere of the "Tamm Department", he was an active member of that wonderful community and invariably saw examples to emulate, and so it's nothing extraordinary that with gratitude (and with good reason) he regards himself as Tamm's student.

I should stress that Tamm's criticism was never with the intention of imposing his assessment as to the prospects of a work his students had chosen. There were quite a few instances when he showed doubts and even serious scepticism about work done in the Department. But such scepticism he expressed very cautiously. This must be why Tamm's students so differ in individual makeup, in areas of research and interest, why there is such a marked distinction in the schools they have set up.

His broad view of possible approaches and assessments when one's belief in the correctness of his own choice is combined with the utmost tolerance of and respect for another's position made it quite natural that his benevolent support for his students and colleagues never, I wish to stress this word, never brought condescension towards work, views and styles of other schools, other theoreticians. There was no sectarianism in his attitude to research outside his range. He took an objective view of such studies. His joy when he came across a new, substantive work was obvious and instructive for all who witnessed it. His customary restraint disappeared. And if his reaction did show restraint, it meant that although he had found no direct errors, he queried the results or was convinced that the prospects of the study were poor, that he found it useless, though it was of course impossible to prove that he was right. Only a flagrantly anti-scientific approach or pseudo-science aroused him to furious (though cultured) attack. His scientific patriotism never turned to jingoism.

* * *

Someone might ask what was so special about the man. Was he not just a kind man of proper behaviour endowed with talent as a scientist? Do we not find such features in Einstein, Bohr, Mandelstam and many less prominent scientists, and in some not prominent at all? Yes, in point of fact, that was the case. Nevertheless, it seems to me that this concept of ethical behaviour could achieve specific coherence only in a specific medium, in a specific epoch, and namely among the best strata of the Russian working intelligentsia in the late 19th and early 20th centuries, and that it has come down to us from them. But it certainly had no exclusively national character or strictly limited social environment, and so it is not surprising that we come upon many people with such natures. As far as Tamm was concerned, alongside these features he was a man of rare integrity and thus set a standard to live up to.

I have tried to give an insight into some basic aspects of Tamm's personality and illustrate each with facts from his life. Reading over what I have written, I feel that I have not altogether succeeded. Some facts that demonstrate his independence of spirit could be attributed likewise to his courage or adherence to principle, and vice versa. This is not accidental. The point is that all Igor Evgenievich's features merged in one integral, albeit amazingly complex, nature. The charisma, experienced by all who came in contact with him, cannot be divided into components and reassembled piece by piece.

His life passed through various stages. Initially, somewhat during a quarter of a century, he moulded his outlook and searched for his way in life. The ensuing thirty to thirty five years were a period of immense scientific productivity (especially in the first two decades), against a backdrop of growing recognition from colleagues, and personal misfortunes. And finally, the last decades were marked by broad social, official and scientific recognition and respect, by what is customarily called glory. But both the hard times and the glory brought features of his personality into bolder relief, those features which, like the radioactivity laws of nuclei, could not be changed under the influence of external forces. Surprisingly, they had a profound appeal to people, even to those who had never come into direct contact with him and were not in a position to estimate properly his contribution to science.

I. M. Frank

REMINISCENCES OF DIFFERENT YEARS[1]

I find it difficult to speak about Igor Evgenievich Tamm. A host of memories flood through my mind, and it is not easy to sort the essential from the secondary and the not worth mentioning. A small article risks too much of the generalisation that distorts the image. I have vivid memories of Tamm in his last year, when he was ill, and this is inevitable for in that tragic hour I arrived at a clear realisation of what a wonderful man he was. I can remember the beginning of our acquaintance in my student years almost equally well. Some 45 years fall in between, but the human memory is a marvellous thing, and it does not consider distances in time. You know in your mind that a young man's perception of the world as he is making his first acquaintance with science and scientists, differs from what he feels decades later. Tamm himself had no doubt changed in those 45 years, though we might not have noticed it. So I shall give a brief account of the impressions of my youth at the end of my story.

I may well not be alone in recalling Tamm's long illness. Everyone who called on him in those times must remember the stertorous breathing of his iron lung you could hear the moment you entered his flat. It made your heart sink. On reaching his room you saw Tamm in his bed, smallish, wizened, chained to the machine, but brightening with the pleasure of the meeting. You felt horrified by his situation, but yet you felt comfortable and at ease enough to forget how hard it was for him all the time. Yes, things were tough indeed. When he spoke, he often got out of breath, and a nurse had to be called in to add air using manual equipment. Yet I never heard even the slightest murmur of complaint — he would carry on talking, joking, show an interest in everything. His spirit was not broken.

I had something stored up to tell him every time I went to see him. It might be the ultracold neutrons in which he was so interested, or perhaps my impressions of Mongolia. He was never a passive listener, always asking this or that, voicing his opinion and often coming forward with advice. If I may use some

[1] First published in the *Vestnik Akademii Nauk SSSR* (1978, No. 7, pp. 105—115) under the title "Igor Evgenievich Tamm — Reminiscences from Different Years" (in Russian) — *Ed.*

high-sounding phrases — indeed I find it hard to avoid them — I might just as well say that he had a placid wisdom accompanied by geniality. And his joy at our meetings was more than just pleasure at someone coming to see him. That joy exuded the warmth of his heart, and I realised and deeply appreciated that over the 45 years of our acquaintance he had not changed his attitude towards me.

That does not mean, however, that Igor Tamm became permissive. That was not so; but he did speak his mind in a much milder form. One day I, thinking it would be pleasant for him, mentioned a young physicist who had always revered him. I heard in reply: "Yes, he is a very vigorous young man." And a few minutes later, he repeated with a smile: "Yes, most vigorous indeed." I understood that I had better drop it.

Asked about a scientist who had worked rather closely with him for some time, he told me, almost apologetically: "You know, I would rather not see that man if I can avoid it." Yet he might voice his profound respect for a man who, I believed, could be rash to make harmful decisions and who was very biased against others. "That man has a good soul," Tamm added to my great surprise. And in the long run he proved right.

Living in the shadow of death, Tamm took an unemotional view of people, an outlook free of the subjectiveness born of superficial or chance impressions. He looked at the heart. In short, Tamm was evolving into the same well-nigh legendary Tamm who lives in the memory of those close to him.

Tamm could not tolerate any manifestations of pseudoscience. Yet the way he fought it changed with the years. Many will remember his implacable stand against the erroneous attitude to genetics. Genetics was within the realm of his scientific interests, and he would speak with enthusiasm about a genetic code, remarking that it gave a physicist much food for thought. He could not endure pseudo-scientific biologists. I remember an incident which occurred about 15 years before his illness. I went for a break to the Academy of Sciences' health resort at Uzkoye where Tamm was also holidaying. From the cloakroom, while I was taking off my things, I saw him trotting past at his usual brisk step. He glanced in my direction but went on. I paid no attention because he obviously could not have recognised me in the dark hallway. A few minutes later we met in the dining-room and he greeted me as warmly as usual. Asking whether it was me whom he had seen in the hall, he was most apologetic, said he had not recognised me, and I could see he was sincerely sorry. I could not understand what it was all about until, embarrassed, he explained that he had taken me for X., who was also

staying at Uzkoye. X. was among those who "barked" at genetics. "You see," Tamm said, "I would rather not stop and be obliged to greet him." When X. died his name was so quickly forgotten that there is no point now of giving it in full.

In the last two decades of his life, Tamm used all his prestige to expose mistakes and whatever hindered the development of Soviet science and fair treatment of real scientists. He fought injustice in every its manifestation. As to mistakes in science, I think that in his earlier years he at times levelled his criticism at objects obviously not worthy of his attention. I remember that back in the 1930s he slated the theoretical works of one university professor. The critique must have been fully justified. I say "must have been" because the works have long fallen into oblivion. An erroneous work, as a matter of fact, is less damaging than at first glance one might think. For if it is not boosted and used against correct works, it has a way of just dying a quiet death.

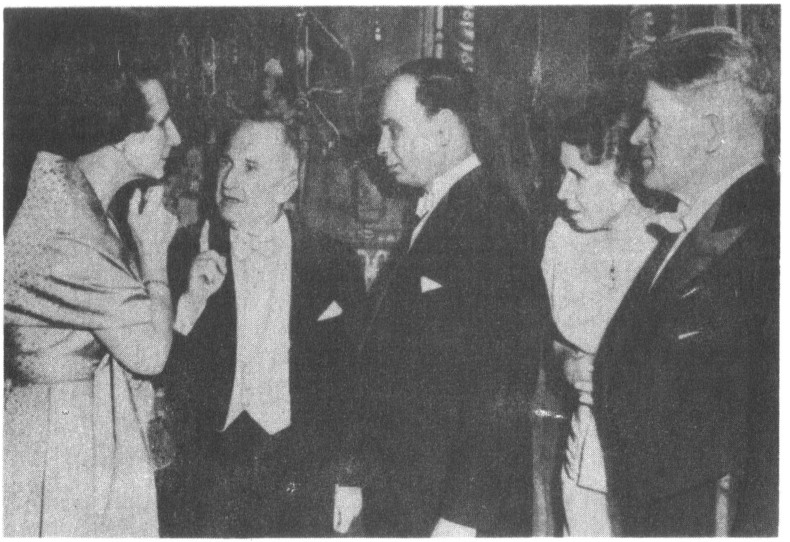
Was Tamm always right in his assessment of other people's works? Like everyone, big scientist or not, he could perhaps err. But that is all beside the point now; other, essential things should command our attention.

* * *

Understandably, I remember well the time we spent together in Stockholm in 1958 at the Nobel Prize ceremony. It was then that I gained an insight into Tamm with all his appealing qualities and some of his foibles about which I now recall with a smile.

It was only natural that we should be together most of the time. In the Grand Hotel where we were staying, I often called in on him to talk about science or the day's events. On his rare free evenings Tamm would lead the Cherenkovs and me to a youth cafe nearby for our supper. It was near Christmas, and there were parties on all the time in the hotel. But it was cosy and quiet in the cafe which one of the Swedes had recommended him. Being the only one of us with a good command of English, he was going through the menu choosing our supper for us and doing it with much relish. There was no orchestra, but there was a grand piano, and the pianist always played something Russian for us, mostly Chaikovsky. Our pictures had been in the papers, so we were recognised everywhere.

Tamm invariably dominated the conversation at our table, telling us what he had heard or read in the press, or just recalling an interesting thing. We relaxed as we talked, all of us could have called it a day, except Tamm, but I shall go into that separa-



I. E. Tamm, I. M. Frank and P. A. Cherenkov after receiving their Nobel prizes (1958)

tely. In all fairness I must say that it was Tamm who most needed rest, because he had been particularly active throughout the day. He never missed an opportunity to discuss scientific problems with any scientists he happened to meet. He had spent a lot of time with our American colleagues George Beadle, Edward Tatum and Joshua Lederberg who had received prizes in physiology and medicine for discoveries in genetics and gene properties, and he naturally had wasted no time in striking up friendships with Swedish physicists.

There was another side to our Swedish visit. Now that contacts with our country have gained ground and Soviet scientists' participation in all world science events had become customary, it is hard to conceive that three Soviet Nobel Prize winners appearing in Sweden in 1958 looked like *corvi albi* (white crows); or should I rather say polar bears. Some photographers took our pictures in the zoo against the background of polar bears, to make the impression that bears were part of Russian life. A literature prize awarded to Pasternak in the same year (1958) also contributed to the generally cautious view (though not of course on the part of our colleagues in science).

We were clearly aware of all that since we had to meet many people, including journalists, and even take part in news-conferences. At such meetings Tamm would take the initiative, using all his natural charm to win his interviewer's confidence and friendliness (and why not). As a matter of fact, he did an important job, and in time people warmed towards us. The interest of the press petered out of its own accord, as film stars and murder stories returned to the front pages. Our faces were forgotten, and we became ordinary foreign tourists, which we frankly much preferred.

The press wanted to make an issue of our visit in 1958 and I found fresh proof when I came to Sweden in December of 1975. Over 70 Nobel Prize winners in science, including six from the Soviet Union, gathered in Stockholm on the 75th anniversary of the institution of Nobel Prizes. But the papers did not show that much interest even in Academician L. V. Kantorovich, the 1975 prize-winner, at least no greater interest than in his colleagues from other countries.

This does not apply to the organisers, the Swedish scientists, whose attention, cordiality and hospitality were just wonderful both in 1958 and in 1975. Tamm made an abiding impression, and I could still feel it 18 years later when I met again some of the scientists with whom I became acquainted in 1958. Warm recollections about Tamm from all who had known him came as no surprise to me. It seems that even their genial attitude to me stemmed to some extent from the fact that our two names were linked.

Tamm's lively interest in science, which did not flag even in the festive atmosphere of those days, was particularly remarkable. That wonderful feature of his nature really amazed me, particularly in those days in Sweden. One of the Swedish professors happened to tell about a telephone conversation with someone in Denmark who had heard second-hand of some new results in elementary particle physics. Those must have been preliminary, tentative results, and no one knew the details. Moreover, the impression was that we were dealing with some mess, some story fed into the grapevine by people not directly involved in the work. Yet for Tamm it was an overriding incentive to get down to theoretical description of the problem without any delay.

Each evening he would avidly set to work. Neither the lack of corroborative evidence nor the bustle of receptions and meetings played any part. Returning to the hotel, he would resume his work immediately and sit up late into the night rapt with concentration. I knew what it meant as I recalled the sleepless night

I had spent at the desk with Tamm twenty years earlier.[2] Each morning I found Tamm in his room upset about the poor results he was getting, sitting at a desk piled with sheets of paper covered with notes. I knew his sheets of formulas, written illegibly in his large hand. Tamm told me once he had two kinds of handwriting, one for his draft notes, and the other for clean copies. On one occasion, as we were engaged in a joint work, I saw him at it, and it was more than just making a fair copy of what he had done before; it was the creative work of rethinking the results, of bringing them into logical sequence and, what is most essential, elucidating the physical facets of his calculations. Fig. 1 reproduces a page of draft of our joint paper copied into my notebook in Tamm's hand.

This is the kind of night work that I meant when I said that all of us retired to rest after supper in the cafe, except Tamm. His attempts that yielded no positive results continued for a number of days, perhaps a week. I tried hard to persuade him to put that work aside because the subject-matter was not clear or perhaps was non-existent as it subsequently turned out. All Tamm would say was he could not help thinking about it, that it was essential to him to understand if such a result was possible and if so, what the sequel could be.

On one occasion that ardor for work really unnerved me. We attended a reception given by some Swedish physicists where one could talk to many people about a lot of things. Suddenly — it was still early — Tamm said he would like to go back to the hotel. We were using the same car, so we had to go together. I did not realise that he was so eager to leave immediately, and anyway I could not go right away. I was involved in a discussion with a young physicist, and it would have been ill-mannered to break off the conversation like that.

When a little later we had finished, I looked for Tamm, but I could not find him in any of the rooms. Then I went downstairs and saw him in the cloak-room with his coat on. "I have been waiting for you for a whole ten minutes," he said with a degree of irritation that I cannot remember in decades of our acquaintance. Of course I was upset and even offended, but now I am inclined to think that he had just had a new idea and was impatient to get down to work. There might have been something essentially good in his attitude towards me: at that moment I was a former

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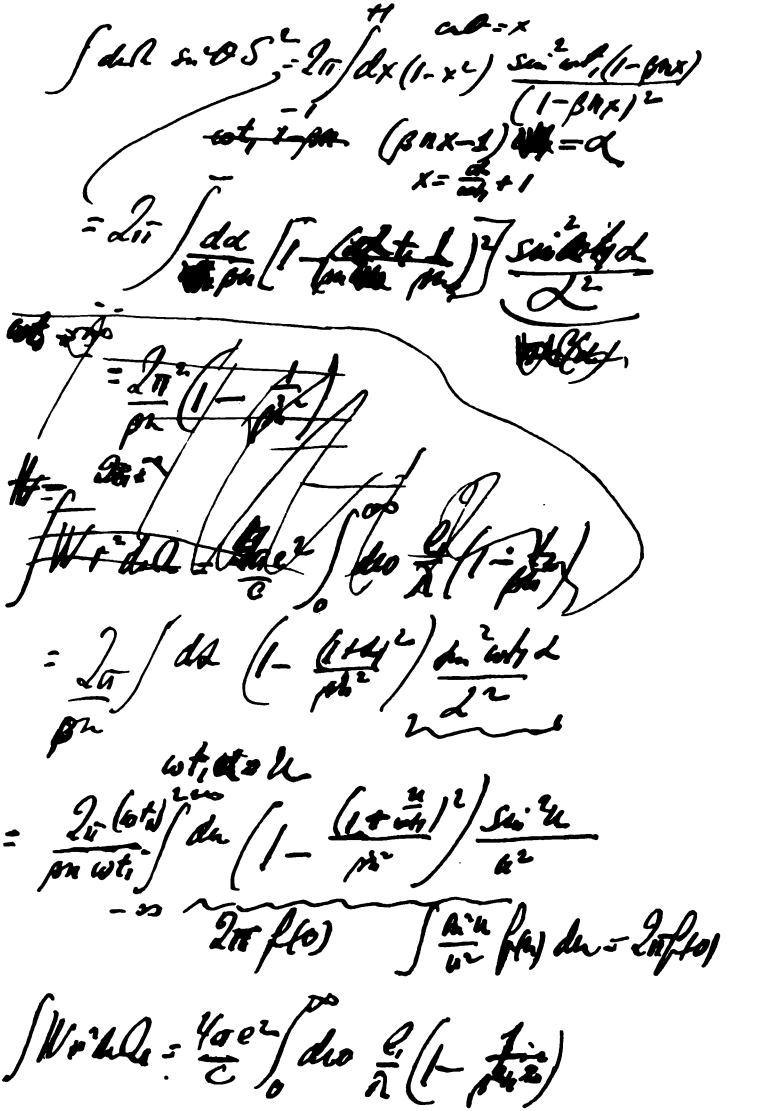
[2] I told about that in an article for the collection dedicated to I. E. Tamm's memory: *Problems of Theoretical Physics* (Moscow, Nauka Publishers, 1972, pp. 362—363, in Russian). See also the present collection, p. 156.



One of the pages where Tamm deduced the formula for the energy radiated by a moving electron

student and pupil accompanying him. The point is that a teacher's regard for his student and vice versa, if years have not darkened their relations, remains subconsciously the same, even when the student is no longer young and has pupils of his own.

Tamm's keen enthusiasm in those days, when we were so close, led to mishaps now and then. I remember we had to deliver our Nobel lectures at the same session, one after the other. The content of the lectures meant Cherenkov was the first to take the floor with Tamm next and me last. Nobel lectures are not distinguished by any overly festive atmosphere; they cannot be compared, for example, with the speeches made by Lomonosov Medal laureates at a General Assembly of the USSR Academy of Sciences. The atmosphere is much more modest. Nobel lectures are read in a student lecture-hall, with a few Swedish professors attending who have to be there by virtue of their position. Most of the hall is filled with students who come mostly to take a look at the lecturers. In other words, the situation is similar to a regular college seminar at which a famous professor is to speak. As far as I know, the situation has not changed. In December 1975 I heard the Nobel lectures of Rainwater, A. Bohr and Mottelson, and it was much the same, except that the young King of Sweden attended the first two lectures. I do not know whether this has become a tradition or if it was a tribute to the famous Bohr. The Queen of Denmark also attended the 1975 ceremony.

As to our lectures, Cherenkov read his text in German in exactly 40 minutes. (If I am not mistaken, we each had 40 minutes). But Tamm, having begun his lecture, got carried away and, clearly forgetting that it was a Nobel lecture and not a seminar, strayed from his prepared text and went into a number of interesting matters outside his topic. I saw that he would not keep within the time limit. He noticed it too.

Then something unexpected happened. He turned to me all of a sudden and asked: "Could you give me ten minutes or so of your time?" Had it been a regular seminar, there would have been no need to ask — I would have simply postponed my report, at least some part of it, until the next meeting, just for the sake of hearing Tamm. But this was different — I could not do as I pleased, I could not even shorten my lecture. Having no good command of English, I was tied to the scripted text rehearsed in advance, and I was even afraid that I might read it more slowly than at home and not fit into 40 minutes.

It was my first major speech in English, and such a responsible one at that, and I was very nervous. On the eve of the lecture, I had read out some of the pages for Tamm to tell me if I was making any bad blunders in pronunciation and how it

sounded in general. Tamm had encouraged me and said everything would be all right. But during his lecture he must have forgotten about all that. And when I failed to reply to his request, he repeated it again, frightening me out of my wits. Naturally, everything passed off all right since the chairman set no limit to Tamm's time and did not curtail mine either. It was the audience that was "punished" by sitting an extra 20 minutes which was not altogether common for the punctual Swedes.

After my lecture Tamm praised me and said that it was interesting and understandable, though I had mispronounced a few words. In general Tamm was attentive and considerate to me, and I felt his kind regard at that time more than ever before. And in retelling that incident, I seek merely to show with what abandon he threw himself into work or discussion. He could forget about everything else.

Tamm had a great sense of purpose. It seems to me he showed little interest in the routine problems of physics where solution simply contributes to the theory of this or that phenomenon. His in-depth understanding of physics and his brilliant mastery of mathematics meant he could have gone far with such investigations which were useful and interesting, but his interests lay elsewhere.

For all that he was invariably aroused when an experiment revealed something cardinally new or when he developed fundamental theoretical problems requiring solution (on nuclear forces, for instance) or grappled with a theory's fundamental difficulties. He never shunned complex tasks where the probability of failure was high. If some new idea caught him, he never doubted he was on the right path, and setbacks could not but upset him. He would tell me about them now and then adding: "Never mind, it's not the first."

I spoke about Tamm's courage in the face of such failures in my funeral oration.[3] Yet it now occurs to me that I never heard from him what an effort and how many sleepless nights any one of his best works cost him.

Since I have touched upon my stay in Sweden, I really should come forward with some story on the Nobel Prize ceremony. But here I find it difficult to keep within what concerns only Tamm. So with this reservation in mind, I think I can give one short account.

First of all, we had to put on morning coats. We all knew

[3] Published in the book *Igor Evgenievich Tamm. Collection of Articles.* Moscow, 1973 (in Russian).

about this mainly from Laura Fermi's biography of Enrico Fermi. She described it nicely, with a journalist's touch. Tails are perfectly good clothes, nice, severe and even comfortable once you get used to them, but that's just the snag: scientists are not used to tail-coats. It is marvellous to see musicians so attired, but wearing them yourself you feel like an actor all dressed up for an unusual stage role.

This impression was all the stronger because the Nobel award ceremony was held in a large concert hall and our entrance was also a piece of theatre. Dressed up in our tails, we stood backstage, each with a counterpart, a member of the Swedish Academy as escort. Cherenkov was in the first pair with Karl Siegbahn, next came Professor Eric Hulthen and myself, and then Tamm with Professor Ivar Waller. We waited the signal. It may be that I was shifting from one foot to the other because Tamm stretched out his hand, patted me on the shoulder and said: "Don't worry, it will be alright." But I could see that he too had butterflies.

On emerging on the stage we bowed to the audience in the hall as instructed (though in actual fact it was to the King of Sweden who just stood in front of us) and flopped into our armchairs. It was at this point that it dawned upon us that we were sitting while all of the people in the hall, a glitter in festive attire, ladies in evening dress and the King in the middle of the front row, were on their feet, rising before us in a pageant wall. We glanced around in fright, squinting towards our American colleagues and calmed down, for we saw that they were also seated: we were not the only ones to breach etiquette, if that was what we were doing, but we were not.

In later years, as the concert hall could not take in all the guests (with so many invited and willing to come), Nobel Prize ceremonies were moved to a roofed-in stadium, and the ritual was changed.

In the evening, upon the presentation of the Nobel medals and diplomas by the King, a formal dinner was given at which the laureates, with the King and the Prime-Minister and the royal family, celebrities and prominent scientists, sat at the central table. A prize-winner from each of the groups made a short address, with Tamm naturally representing our group.

We approached the table in ceremonial procession: everyone came with his lady on his arm (it is taken for granted that both the lady and the seat at the table were known in advance). My partner was the King's granddaughter Birgitta, a charming young girl. On downing a few glasses of wine, I began to feel my feet in the capacity of the princess's gentleman and produced some small talk in my "Pidgin-English".

Tamm was sitting almost opposite me, and I saw on his face that he was openly envious of me. The point is that his lady was not of the royal family and older than mine. Of course he should not have felt bad about that, since he was with one of Sweden's foremost ladies. Be that as it may, Tamm had an opportunity for revenge at the next reception. He had the Queen of Sweden on his right and the young princess on his left. Tamm beamed as he entertained them both. I rediscover these boyish traits of Tamm's nature again and again as I recall the early years of our acquaintance, three decades before that.

* * *

And now I would like to turn back to my college days, to memories that warm the cockles of the heart. I remember my father, just back from Simferopol where he had lectured on mathematics, introducing me to Igor Tamm just after I first went to Moscow University in 1926. My father had worked with Tamm at the University of the Crimea some years earlier and they had kept in touch. Many eminent mathematicians, physicists and biologists treated my father with great respect. He was a talented mathematician and a wonderful man. I am very grateful to Tamm for what he did for us in the wartime years, as my father lived out his days in Kazan upon our evacuation from Leningrad; despite the rigours of the wartime, he found ways of giving substantial assistance to both of us.

So Tamm entered my life as my father's friend; I was his friend's son. Later I came to know Tamm as a university professor whose lectures I attended, but I know next to nothing about his research in the 1920s. We continued our personal contacts. I was a frequent visitor to the house of professor Alexander Gurvich, a well-known biologist under whom my brother was taking a postgraduate course. My father had made friends with Gurvich and his family at Simferopol, and so I met the family too. Gurvich's wife Lidia, a talented biologist, was very motherly towards me and I often — and at one time regularly — had dinner with them, something, to tell the truth, I really needed at the time. I often met Tamm at their home.

Although not a Communist Party member, Tamm enjoyed the reputation of a Bolshevik (Communist) among the faculty, few of whom were party members. He impressed me as a superb storyteller. Most frequently of all, he would tell us about his adventures during the Civil War when he got into the most fantastic scrapes. The Whites mistook him for a Red spy, but his courage,

resourcefulness and ingenuity got him out of some really hopeless situations. Of course I believed every word and even now I am inclined to think that in substance his tales were true. He was no doubt a very brave man. When we received our Nobel Prizes, he told me about a letter he had got from a man he had saved by carrying him, wounded, from the battlefield. But these stories also revealed Tamm's boyishness. It never occurred to me at the time to commit Tamm's tales to paper, and it will be a great pity if no one has done it.

One day I called in at Tamm's. I don't remember why exactly; perhaps it was to borrow a book that he had recommended. His living accommodations appalled me. If I am not mistaken, he had no regular study — what he had was a cubbyhole with a board partition where his table stood, with reams of manuscript on it, his famous book *Fundamentals of the Theory of Electricity.*[4]

In my article about our work together I told something about the Moscow University Chair under Mandelstam where Tamm worked; and I told about his lectures. As to the lectures, I wrote that I could not "remember anything... save commonplace academic jokes about professors and students".[5]

Student jokes often played up Tamm's extraordinary liveliness and agility. Once in a wall-newspaper he was depicted as a vague blur, symbolising a wave packet, at the blackboard. It was difficult to put down his lectures, for he moved about and talked very fast. Hence the funny unit for speed of speech, the tamm, that gained currency among us at the time. Since this unit was on the big side, it was suggested that speed of speech should be measured in millitamms.

As almost 50 years have elapsed since then, it is only natural that the notions associated with the word "Tamm" have changed. D. Danin's fair hand made the tamm a unit of decency, which was something we all took for granted. In poet Block's words, as applied to our story, time has erased the casual features and shown the magnificent outcome of the life of Igor Evgenievich.

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[4] It took pretty poor lodgings to surprise me at the time. I had no permanent refuge myself, but I didn't mind: I rented a bed in a lodging-house, slept on a sofa in a dental surgery and even on a table in the reading room of a children's library when it was closed.

[5] *Problems of Theoretical Physics,* p. 352.

COHERENT RADIATION OF A FAST ELECTRON IN THE MEDIUM[1]

More than 40 years have passed since the article "Coherent Radiation of a Fast Electron in the Medium" (1937) was first published. For most present-day readers it is just an episode from the history of physics. But what today is perfectly clear and even obvious was not so forty years ago. As is usually the case the original premises which posed the major challenges had later undergone changes without leaving noticeable traces in literature. With the passage of time they pass into oblivion. So perhaps it is worthwhile to recall the details and to tell the whole story.

I realise that no matter how hard I try to avoid it, the story will inevitably be a bit subjective and one-sided. Also, I am embarrassed by the fact that this article dedicated to the memory of I.E. Tamm will be, in a certain measure, autobiographical. In self-justification I would like to say the following. An analysis of your own scientific career at an age when it has been so long makes you understand much more clearly the importance of the continuity of a relationship with the past, especially with the scientists with whom you have rubbed shoulders and worked with. Tamm's influence on me was immense, even apart from our joint work about which I am writing. Our work in fact determined my major field of research since then.

In my student years I was lucky to get into Mandelstam's school, a truly intensive and diversified scientific environment. My teachers there included such outstanding physicists as S. I. Vavilov, G. S. Landsberg and I. E. Tamm, each of them a distinguished and highly individual personality. Yet there was one thing common to the whole school: unceasing scientific communication and exchange. Always there was discussion under way of theories and experiments; these frequent, long exchanges in seminars and outside them were never regarded as a waste of time. At first I was surprised that such prominent scholars spent so much precious time talking, often about failures and even seeming nonsense, so much time that in my opinion could have been used to more advantage. At that time I did not realise that frequently they were hammering out new concepts and ideas long before publication (and of course, there was no fear of plagiarism). Everybody was eager to help formulate a new concept without any notion of being a co-author. This was only natural in the moral atmosphere that reigned at Mandelstam's school.

Continuing discussion of new projects and related ideas with colleagues and disciples was characteristic of Vavilov too. It was natural that I was in the know about P. A. Cherenkov's experiments in every detail from the very start. S. I. Vavilov had introduced me to him early on. Our close cooperation began as soon as I joined the staff of the Lebedev Institute. After 1934, when the Academy of Sciences moved to Moscow, Cherenkov's experiments came up for discussion more than once between Vavilov and Tamm. I was also in touch with him. Without

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[1] Some fragments from I. M. Frank's paper on "Coherent Radiation of a Fast Electron in the Medium" are given here with a few notes. The author makes frequent allusions to this paper in his previous article (which is published in full in the book *Problems of Theoretical Physics: To the Memory of I. E. Tamm.* Moscow, Nauka Publishers, 1972, pp. 352—378). The article tells of the author's work with I. E. Tamm for which they were jointly awarded the Nobel Prize — *Ed.*

these repeated discussions the research I am writing about would hardly have got off the ground.

Now a few words, purely autobiographical, about my own studies of electrodynamics. Shortly after I was admitted to Moscow University (in 1926 or 1927) I asked Tamm's advice on what to read in physics. He recommended Abraham's *Theory of Electricity*. It had not yet been translated into Russian (or I could not get a copy) so I had to read the German edition. Probably Tamm's advice was incidental to his writing at that time the manuscript of his now famous book *Fundamentals of the Theory of Electricity*, but it was very important to me. I had inherited a liking for geometry from my father, a mathematician, whose great influence on me probably explains why I went in for the field theory. A small book by Maxwell on Faraday's lines of forces which I came across in the students' library of Moscow University's Department of Physics and Mathematics was a real eye-opener to me.

At the university I attended Tamm's lectures on theory of electricity and he examined me in the subject, but this did not push me towards independent research as yet. I cannot recall anything special, just the banal stories about professors and their students. Then for a long time I lost sight of electrodynamics. By a long time I mean six or seven years, a lot for a twenty-year old beginner in physics.

My return to these problems was connected with Cherenkov's work in 1934 and the research in nuclear physics I was doing on Vavilov's advice. Vavilov's interest in Cherenkov's experiments got me interested too. I shall remind the reader that Cherenkov used a visual method for making his measurements. Both weakness of the glow and the use of the visual threshold sensitivity photometry method ("quenching method") took a long time for the eyes to adapt to the darkness. It was difficult to take these measurements without an assistant and sometimes I chanced to be the one. As a result, I had a good idea of what was being done. And it is quite natural that I usually as well took part in the discussions of the results and the planning of experiments alongside Vavilov.

And now I shall attempt to sum up the situation early in 1936 concerning a problem which was decisive in understanding the effect. A good deal was already clear from Cherenkov's first publication in 1934. In studying the gamma ray-induced luminescence of uranium salt solutions Cherenkov noticed a faint glow in the solvents whose origin seemed obscure. This led to a series of studies on the light emission by pure liquids exposed to the gamma rays of radium (in solids it could be normal luminescence). Light emission proved to be a general property: every liquid examined radiated at virtually the same intensity (within 30 per cent). Light filter measurements indicated that the radiation spectrum of different liquids lay within the same margin of error. It covered a wide range of frequencies and the colour, where it could be seen (at low intensities it could not) was blue. And though at that time it was not possible to see the colour Vavilov confidently entitled the paper, he published alongside Cherenkov's first article, "On the Possible Origins for the Blue Gamma-Induced Radiation of Liquids".[2]

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[2] I feel that the articles by S. I. Vavilov and P. A. Cherenkov ought to be regarded as two parts of the same work: experimental and theoretical. As to the colour of the radiation it turned out later that its spectrum was known though none of us was aware of it. The spectrum was photographed by Mallet who observed (1926—1929) the radiation of several liquids and noted its peculiar properties. Today water's blue radiation is usually shown to visitors to atomic reactor facilities.

The fact that the radiation was noticeably polarised and the predominant direction of the electric vector coincided with that of gamma rays was particularly surprising. This polarisation and the fact that the intensity of the radiation remained unaffected by a change of temperature, or by adding a luminescence suppressor had already been established in Cherenkov's first paper. This led Vavilov to the conclusion that the radiation could not be due to excitation of molecules of the liquids; it is emitted by a Compton electron as a result of its interaction with the medium. The only radiation mechanism that seemed possible was Bremsstrahlung and Vavilov assumed it was the case. This assumption at once explained the universality of the radiation and the polarisation, because in a Compton effect electrons move at an acute angle to the primary photon beam. There were no doubts either as to the similarity of radiation spectra in different liquids as these were determined obviously by the Bremsstrahlung mechanism.

... Still, Vavilov's Bremsstrahlung hypothesis was vague in many respects and for a number of reasons it raised doubts. I certainly accepted his view that it was electronic emission and not liquid luminescence. It was far from being generally accepted, however, at the time which accounted, probably, for an obvious lack of interest in Cherenkov's experiments except by a small group of Vavilov's associates. I remember an outstanding physicist remarking: "Well, at the Lebedev Institute they are busy with the radiation of some mud." It was quite clear that further progress required a direct proof of a relationship between radiation and fast electrons.

Of course, the most direct way was to observe radiation from a beta particle source. Now this may seem strange, but at that time it was not so easy to do at an institute that had no radiochemical laboratory. The experiment was carried out in 1936. A radium preparation in an ampoule of thin glass was used. It was shown that the radiation had all properties peculiar to gamma ray-induced radiation. As expected, the luminousity of beta-excited radiation was in inverse proportion to density. An attempt was made to compare the results with the theory, and the conclusion was that there should be a dependency on the index of refraction of the medium.

It was a happy coincidence that earlier in 1936 an indirect experiment had been made to check the role of electrons, which accidentally revealed the most characteristic feature of radiation — its directiveness. Had the directiveness been unknown it could easily have been missed in experiments with electrons as at that time it was not a simple thing to achieve the required electron beam collimation. The aim of the indirect experiment was to show that polarisation in gamma-induced radiation was definitely related to the direction of the movement of the electron. Obviously, this could be achieved by placing the liquid in such a strong magnetic field that the linear path of the electrons should become a noticeably curved arc. The resultant polarisation plane should then turn at an angle towards the deflection.

I remember that we discussed the planning of experiments and later the results with Cherenkov. There was a danger that a considerable scattering of electrons could make them uncontrollable by the magnetic field. The experiment was nonetheless meaningful, and it was a success, but the result was unexpected. Neither Cherenkov nor I remember the original plan of the experiment, but we remember the result very well. The main thing that happened after the magnetic field was switched on was a considerable change in the brightness of radiation, not the turning of the polarisation plane (though obviously it was turning).

... A close examination of the results of the experiments led to a paradoxical conclusion: the radiation into the forward hemisphere with respect to the electron's velocity was much more intensive than into the backward one. And the asymmetry had to be considerable because when the electrons were deflected towards the observer by the magnetic field the luminosity increased noticeably, while when deflected oppositely it waned. I remember that both Cherenkov and I agreed

with this conclusion. Probably we accepted it so easily because our knowledge of optics was inadequate. Because of his great competence in the field Vavilov for his part thought at first that the conclusion could not be correct. A direct experiment performed shortly afterwards unambiguously proved, however, that the radiation was actually asymmetrical. A liquid-filled tube with flat windows at face planes was used which made it possible to observe the radiation in two opposing directions. When a radium preparation was placed at the side against the centre of the tube, the radiation in both windows was of equal intensity. With the magnetic field switched on, the luminosity in the window towards which the electrons were deflected increased, while waning at the other window.

Naturally, it was well known at the time that the Bremsstrahlung of relativistic electrons is directed forward and there was probably good reason to believe that this property was manifest in our case too (in one of his papers Cherenkov draws analogy with Bremsstrahlung). Yet Vavilov claimed (alluding, if I am not mistaken, to Sommerfeld) that Bremsstrahlung for photons of small energy should not have a forward directiveness. And, indeed, at that time they did not know of any directional light emitter and presumed that it was not accidental. Today it is difficult to trace back the reasons for this erroneous opinion which obviously was generally accepted. Nowadays the discovery of directed radiation would probably be seen as an argument in favour of the Bremsstrahlung hypothesis; at that time, however, it encouraged the search for some other explanation that led actually to a correct understanding of the phenomenon. And, indeed, the directiveness of radiation could be explained only if the length of the emitter would be of the order of the wavelength. Such an emitter can be regarded as a combination of point-like multipoles which are coherent and distributed along a certain length. This is how the directional radiation of radio waves is known to be secured. That is why when I told Tamm about the conclusions arising out of Cherenkov's experiments he said at once: "It means that coherent radiation occurs along the electron's path comparable with the light wavelength." Accepting this interpretation it was relatively easy to draw the scheme which is now usually used in popular explanations of the Vavilov-Cherenkov radiation mechanism.

Though this graphic explanation is general knowledge today a few words must be said about it for this story. The underlying formula, as we know, was that of Huygens: every point on the path of a charge moving uniformly and rectilinearly with the velocity v is the source of a spheric wave emitted at the moment the particle passes through this point. When the refractive index of the medium is n and

$$v > c/n, \text{ i. e., } \beta n > 1, \tag{1}$$

(c being the light velocity in vacuum, $\beta = v/c$), then the spheres have a common envelope, a cone whose vertex coincides with the instant position of the charge. The normals to the surface of the cone, i. e. the directions of wave vectors, form the angle Θ_0 with velocity so that

$$\cos \Theta_0 = 1/\beta n \tag{2}$$

... A great deal was learnt from this qualitative picture of added waves. In fact, only fast electrons, $v > c/n$, could radiate. Intensity of their radiation must be proportional to their path, and inversely proportional to the density of the liquid. Therefore, in accordance with the experiment, the total intensity of the glow for electrons from gamma rays must not depend on the density. (It should be remembered that the number of Compton electrons originating in a unit of volume is roughly proportional to density). And, finally, the picture indicated the directiveness of radiation. At that time it only followed from Cherenkov's

experiment that more light was emitted forward than backward. Today some few people do know that the angle magnitude Θ_0 was not obtained experimentally; on the contrary, it was a prediction that was corraborated by experiment later on.

From a qualitative examination it was obvious that the spectrum of radiation had to be continuous, as the only restriction for frequency depended on the value $n(\omega)$ in formula (2), and in transparent liquids the dependence of $n(\omega)$ on light frequency ω for visible light was weak. It also seemed probable that the electric vector of waves depended on the direction of the velocity of electrons and so produced the correct sign of polarisation. If the waves, whose addition was considered, were real, there would be no doubt about the universality of the phenomenon.

This qualitative picture explained everything that was known about the Vavilov-Cherenkov effect except the intensity of radiation, which made it vulnerable. I discussed these considerations with several theoreticians who began to show an interest in Cherenkov's experiments (especially after the directiveness of radiation had been established), but I failed to make them understand. The main reason was, probably, their inadequate knowledge of the properties of the phenomenon. Both Tamm and I knew more about the subject.[3] Tamm even suggested that I should publish a paper without waiting for a more detailed examination, but that seemed premature. The problem of intensity was left open. What is more, the possibility of radiation was immediately put in question. Tamm told Mandelstam about the qualitative picture which gave grounds for interpreting radiation. Mandelstam's comment could be summed up as follows: it is common knowledge that when the motion of electrons is uniform and rectilinear they do not radiate. The result will not change if c/n is substituted for the light velocity c because in that case one equation is immediately reduced to another if there is a respective change in the velocity of the particle. I did not take part in this talk, but it was obviously only short, and in any case attention was not drawn to the fact that it did not refer to velocities which exceed the phase velocity of light, i. e. to the case that cannot be achieved in vacuum.[4]

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[3] Of course, it was also true of S. I. Vavilov. With a physical intuition characteristic of him Vavilov showed a lively interest in the idea, anticipating its further evolution.

[4] The case of electrons moving at a velocity exceeding that of light in vacuum was investigated by Sommerfeld as far back as 1904—1905. It was shown that if $v>c$ then electrons would be emitting light continuously. When such a velocity was shown impossible by the relativity theory, Sommerfeld's results were forgotten. A. F. Ioffe remembered them when discussing our work before it was published. Later, Tamm studied the analogy of our theory with Sommerfeld's. Sommerfeld was happy to hear of this unexpected development of his theory and sent Tamm a letter in response to our article.

It turned out, as Vavilov discovered, that before Sommerfeld, in 1901 Lord Kelvin had noticed that at superlight velocity atoms had to emit light. He used an analogy with Mach's waves. In reality, in the case of the atom, as compared with a charged particle, the matter appears to be more complicated. With $v>c/n$ the atom must get self-excited, emit frequencies of anomalous Doppler effect and lose kinetic energy.

But Lord Kelvin, as it turned out, also had a predecessor of whom he knew nothing. In 1888 Heavyside had, in fact, predicted the phenomenon discovered by Cherenkov though nothing was known at that time either about fast electrons or radioactivity. Heavyside's work, forgotten by all, was brought back from oblivion only recently — more than 80 years after publication.

Of course, Mach's experiments with a bullet moving at a supersonic speed were known to Tamm and to me. I cannot recall now whether an analogy with Mach's waves cropped up, or whether it was still thought that such analogy could not be applied to electrodynamics. Both presumptions seem now more than strange. Whatever the reason, Mandelstam's comment made in passing acted like a wet blanket. Tamm felt after that that before proceeding further with the proposed physical picture we ought to check if there were some other ways of explaining the phenomenon.

As to myself, I firmly believed in this picture; still, in tribute to the prevailing delusion I was trying to find a microscopic mechanism of the origin of the waves. If no radiation was produced during the uniform motion in an optically homogeneous medium a microscopic mechanism seemed warranted. From the spring to the autumn of 1936 the question remained open.

... Meanwhile Cherenkov's experimental research which gave support to the idea of directed radiation was making good headway. Directiveness had been proved experimentally. Today it is difficult to imagine how surprising the phenomenon seemed at that time.

I recall how Cherenkov's experiment, now to be found in every popular physics book, was demonstrated to Joliot-Curie who came to Moscow in the autumn of 1936. A vertically placed glass cylinder filled with liquid was encircled by a conical mirror. Looking at the mirror from above one could see the angular distribution of radiation propagating horizontally through the cylinder's glass walls. When a radium preparation was placed at the side of the cylinder one could clearly see two maxima of radiation at an acute angle to the direction of the gamma rays. Cherenkov's photos of such rings with uneven blackening at different azimuths are well known today and the experiment is impressive and faultlessly convincing, of course, if no suspicion creeps in of an elementary mistake bordering on a swindle. Obviously such an idea came to Joliot-Curie's mind and he immediately started to turn the cylinder and the mirror around their axis to see if the cylinder's glass transparency or the quality of the mirror's silver plating had anything to do with the effect. In discussing the experiment he intimated an analogy with Blondlot's N-rays,[5] which was not at all surprising as the experiment was being conducted in complete darkness with the light intensity at the visibility limit even after the eyes had adapted. And the whole atmosphere, very unusual for a physical experiment, was more like a seance of spiritism or a trick produced by sleight of hand.

The experiment had been preceded by a period when the theory was not yet complete while the urgency of the task was obvious. This led to a new discussion of the problem with Tamm. Various hypotheses were examined (I cannot remember them now) and all proved abortive. It was clear that the physical picture based on Huygens' principle was the only one that could yield a correct result. Both the $\beta = v/c$ value and the path of the most energetic Compton electrons could actually produce the required directiveness of waves at an acute angle to the electrons' velocity. After this or probably these discussions (I do not remember now how many of them there were) Tamm rang me up one night and asked to come over to his place at once.

I found Tamm at his desk engrossed in a mass of papers covered with formulas. He started telling me what he had been doing. I do not remember exactly what we discussed that night. I think it was the mode of dealing with the problem suggested by Tamm, the calculations, and the physical principles

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[5] Blondlot's erroneous experiments (he thought that he had discovered a new type of radiation) are now completely forgotten. At that time, however, N-rays were a common name. When erroneous experiments resulted in "mysteries" these were qualified as N-rays.

of the theory which was unclear in many respects. The only thing I remember well is that we sat up late and I had to walk home as the transport was not running at that early hour.[6] I had a feeling that an important event had taken place in my life, probably because for the first time I was a party to a theoretical endeavour and with Tamm at that.

When I was leaving for Tamm's I took an excercise book with me and Tamm scribbled down in his own hand the resultant formula for the energy of electron radiation. Quite by chance that notebook has not been lost. The formulas run on five and a half pages and are scribbled in a hurried hand with corrections all over the place. Still, judging by the fact that some intermediary calculations are left out it is not the original deduction, but an attempt to systematise the results. I am attaching a photocopy of one of the pages on which Tamm put down the final formula.[7] The following pages of the notebook are filled with a more detailed deduction of the same conclusion, the formula written down probably later, more accurately in my handwriting. The final formula in the notebook is correct except for the integration limits, but the calculations leading to it are considerably at variance with the one given in the paper we published later.

In line with our experiment the path of the particle was considered limited and rectilinear, and its velocity along that path constant. The field was calculated in the wave zone. Since calculation was performed for a restricted trajectory we could circumvent the seeming difficulty — "a uniformly moving electron does not radiate...".

In our article (dated January 2, 1937) we could already treat the radiation of an electron with unlimited path and eliminated some inaccuracy for small values of Θ (indicated by Mandelstam). The article, therefore, contained a calculation of the energy flow through a unit lengh of the lateral surface of the cylinder whose axis coincides with the particle trajectory.

My memories of this final stage of evolving the theory, writing and editing the article, are comparatively few probably because that was routine, everyday work, except for the seminar at the Institute where Tamm presented the paper immediately after we had obtained the first results. It became absolutely clear to both of us during the discussion that it was senseless to confine ourselves to the case of a limited trajectory and that we had either to admit that electron radiated along its entire path, irrelevant to the existence of the beginning or the end, or that the whole thing was a mistake which seemed unlikely. And this led us to a correct understanding of the problem (Tamm recalled this seminar in his Nobel lecture). ... The theory turned out to be in full agreement with the experimental data obtained by Cherenkov by mid-1936. More experiments in 1936—1937 confirmed the quantitative predictions of the theory as well ...

At first Cherenkov's results and their theoretical interpretation only came to the notice of Soviet physicists. At that time foreign scientists were not extensive readers of our journals, though Reports of the USSR Academy of Sciences (*Doklady*) were published in Russian and one foreign language. In 1937, after everything concerning the phenomenon investigated was clear, Vavilov sent a small article by Cherenkov summing up the results and their comparison with the theory to *Nature*. Under some plausible pretext — I do not remember what — the article was turned down. The true cause was clear: a reputable magazine like *Nature* would not publish results which, at the least, seemed doubtful. The *Physical Review* was less discriminating and published the article.

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[6] It was a pleasant walk, after a strenuous stint, along the Sadovoye Koltso, probably 3—4 km. Tamm lived near the Zemlyanoi Val. My flat was near Mayakovsky Square.

[7] See p. 145.

Soon the first experimental confirmation of the theory came in. In 1938 Collins and Railing in the USA used a beam of relativistic electrons from an accelerator to check the ratio $\cos \Theta = 1/(\beta n)$ on a thin radiator. Possibly the authors of this article addressed Cherenkov's work without the then characteristic distrust because they assumed that the cause of radiation was gradual braking of electrons due to ionisation losses (Bremsstrahlung), which added up into directed radiation. That mistake was quite natural if we remember our own delusions and the fact that Collins and Railing obviously knew only as much of the theory of the phenomenon as they could draw from Cherenkov's article (which only contained a reference to the theory whose results were quoted only to the extent which was necessary for comparison with the experiment). The same authors, obviously, were the first to call the emission "Cherenkov radiation", later a generally accepted term.

The first to take the theory further was V. L. Ginzburg. In 1939—1940 he gave a quantum interpretation of the phenomenon and extended the theory to a case of optically anisothropic medium. The theory was essentially generalised by Fermi in 1940. He took into account the light absorption in the medium and showed the importance of polarisation of the medium for ionisation losses.

Looking back it is probably in place to remember that the radiation of a fast electron in a medium was the first case of a coherent self-luminescent source of light with a much greater length than that of the light wave. Nowadays another example is used to illustrate the possibility of such coherence, the lasers, in which it is really very obvious. This coherence, however, was emphasised even in the title of the article by Tamm and Frank. Later it was used in investigating the interference of light from two thin radiators through which a fast charged particle was passed. No less significant was the fact it was here that it was established for the first time that the optical properties of the medium were as important for the process of radiation as the properties of a fast particle (charge, velocity). Further it turned out that there was a whole class of phenomena related to the radiation of a fast particle, phenomena determined by the optical properties of the medium for which these properties are essential.

It took many years before the Vavilov-Cherenkov radiation gained general acceptance, when experimental techniques and technology made it possible to use this radiation for detecting fast particles (Cherenkov counters).

V. Y. Frenkel

MEETINGS

In the mid-1930s Igor Evgenievich Tamm frequently came on long trips to Leningrad as an adviser at the Theoretical Department of the Physical-Technical Institute. The head of the Department was my father, Yakov Ilyich Frenkel. Tamm usually stayed with my parents who lived next door to the Institute. My child's mind was selective to the world about me so I remember the Tamm of those years as always merry and laughing. I have a clear vision of him in our sunlit dining room in his

beige suit, telling something fast and contriving some fun with me and our spaniel. We all loved him, the children and the grown-ups. And it was to me like a holiday when I heard the grown-ups mention his name or learnt he was to arrive.

I saw the Tamms more often in the years 1941—1943 when Igor Evgenievich and his family were evacuated to Kazan. The Academy's institutes were housed in the building of Kazan State University on Chernyshevsky Street. This rather low but long white-stone building contained the Lebedev Institute, the Leningrad Physical-Technical Institute, and Kapitza's Moscow Institute of Physical Problems. To my mind, never before has the concentration of theoretical physicists been so high in our country. All of them were working in one wing of the university building.

My parents often entertained the Tamms at home. In those hard years, I remember, the Tamms and the Frenkels struck up an especially close friendship with Academician A. N. Frumkin (physicist and chemist) and his wife, who lived in the house of A. E. Arbuzov, a prominent Kazan chemist. Tamm and his family lived in two small rooms of a campus flat. O. Y. Schmidt, P. L. Kapitza and other Moscow and Leningrad scientists also lived on the campus, in one- and two-storey buildings.

I remember one evening with the Tamms very well. Igor Evgenievich sat on a small baby stool, his wife Natalia Vasilievna was doing her housework, and her father, a very old man with a broad and bushy beard, was mending boots. He looked like a real shoemaker as he sported an apron and held small nails in his teeth which he meticulously and rather loudly drove into a shoe sole. When my mother and I entered the room, Tamm jumped up to say hello, said a few words, excused himself and was back at his stool and his notebook.

"Do you mind our talking?" my mother asked.

"Not at all. Please talk as if I'm not here."

"Gora[1] can switch off. Completely," his wife explained.

"You are lucky," my mother said with a sigh. "We are not so. When Yasha works we all go about on tiptoes."

Tamm was an exceptionally charming man. Unfortunately, such statements are not descriptive enough. They can either be unanimously backed up by those who knew him well, or treated as dry by those who did not meet him. Yet just looking

[1] This was how I. E. Tamm's relatives and high-school friends called him.

at his photographs, I think, corroborates such an assessment. Even those still images convey the liveliness and stunning vivacity of his face, the radiancy of his smile, the passion of a narrator or lecturer, and eager attention as an interlocutor. He was not a handsome man, but his face with its deep wrinkles, even when he was still quite a young man, was nevertheless extremely attractive.

The secret of Tamm's charm is revealed even more by strips of newsreel showing him in motion. Fortunately, Tamm became prominent in his lifetime and this prominence coincided with cameramen's growing interest in physicists. A beautiful film entitled *One Tamm* included some material showing the scientist. This was only a first step made by Soviet documentary film-makers to record for following generations Tamm's unforgettable and charming features, his rapid-fire speech, his swift and dashy movements. When I told Tamm that he looked like the American film star Spencer Tracy (known in the Soviet Union for the films *The Nuremberg Trial*, *It's a Mad, Mad, Mad, Mad World*, *The Old Man and the Sea*, and *Gone with the Wind*), he said that he had heard that from other people too.

I also made my personal, though tiny, contribution to Tamm's "family picture album". In 1949 we were on holiday at Majori on the Baltic Sea near Riga. Tamm was also there. I was extremely proud when he invited me to play tennis with him. We spent several mornings playing tennis on a good court we found on Yuras Street. We were equal partners, so the game was exciting. Tamm played recklessly and was highly emotional about good and bad shots, both his and mine. It was then that I photographed him standing by the net, racket in hand, in his whites, a little weary from the battle. What a pity this photograph is of no good enough quality for reproduction.

I also recall going for walks on the shore in the evenings with my father and Tamm. He used to call in on my parents who were at the Chaika rest-house on Alexander Street. From there they went to the beach. The degree of their popularity could be measured in viscosity units: their walk to the seaside was held back, "turbulised" as it were, because they were continually stopped or joined.

Tamm was fond of puzzles — physical, mathematical and logical. He was also fascinated with all sort of mysteries or "horror stories". O. E. and V. G. Goldins, a couple we both knew, were that summer at the same Moscow House of Scientists' tourist camp at Lielupe (near Majori), as the Tamms. On rainy evenings Tamm invited everybody to one room and

then asked his guests to take turns to tell us about some mysterious happening in his or her life. This slightly naive thirst for miracles was characteristic of him, it made him endearingly childlike.

In the autumn of 1956 Tamm spent his holidays at Gagry (on the Black Sea) and invited me to visit him there. When I went to Gagry in early October and found his house, I was disappointed to learn that he had gone back to Moscow two days earlier. The next day I happened to go to the beach of the Writers' Holiday Home. Almost immediately I heard Tamm's name. Playwrights Nikolai Erdman and Mikhail Volpin spoke of him with warmth and respect to the extent of admiration. For several days Tamm's name was on everybody's lips at the Holiday Home which was inhabited by a group of writers. The situation reminded me of a play in which the main character never appears on stage, though the whole plot hinges on him.

When E. S. Fradkin from the Theoretical Physics Department at the Lebedev Institute saw Tamm immediately after his return from Gagry and asked him what it was like there, he said: "Very good! I worked splendidly", and pointed to a bundle of notebooks he had brought back from the Caucasus.

Tamm was, beyond being a very interesting narrator, an eager listener taking all the matters that worried his interlocutor to heart. Meeting him was always a thrilling experience, especially to a young man such as I was then. Such meetings filled my heart with the exultant joy of the miracle of human intercourse, to borrow an expression of Antoine de Saint-Exupéry.

I recall his attentive questions about my electronic valve calculations. At that time I did not know him that well and thought that he was asking questions out of consideration and mere politeness. For this reason I confined myself to just a few words. But Tamm went on:

"No, really, please tell me, what are you working at? You know, I used to study radio valves myself."

And he began telling me how in the early 1920s he worked at a radio factory in Odessa. The factory was very small and his work record there was very short. As regards radio valves, people there had only heard rumours of them. And then some "fool" turned up with a sample valve, a triode. He was given a whole department "for that triode". The man was a real rogue and it had been simply impossible to work with him.

So Tamm and a few friends of his had got a transfer to the Polytechnical Institute where they began working in a "vacuum artel" which was part of the high-frequency oscillation lab.

L. I. Mandelstam and N. D. Papalexi were its patrons.[2] They made rapid progress there. A skilled glass-blower (from the famous Petushkovs dynasty) helped the "vacars" (as the staff of the "vacuum artel" called themselves) make the Langmuir diffusion pump which produced from 10 to 15 valves at a time. Tamm also supervised students in the lab. Those were times when food was short. The "vacars" had got daily a glass of flour for their work and on their way to the lab would make a quick stop at the local market to trade the flour for other food.

"So, we are colleagues, as you see, and I'm really interested in your work," Tamm wound up.

* * *

Our talks also included some strictly business matters. These were connected with three physicists — my father, Paul Ehrenfest and Tamm himself.

The first such case was back in 1953 when we worked on Y. I. Frenkel's collected works, and then on a March 1962 special issue of the *Uspekhi Fizicheskikh Nauk* journal (Achievements in Physical Sciences) to mark the tenth anniversary of my father's death. (The relevant decision was made by the Department of Physics and Mathematics of the USSR Academy of Sciences on Tamm's proposal.) He asked me to prepare some material (mostly excerpts from my father's correspondence) to be incorporated into a big biographical article he was to write for the issue. When Tamm read it, he said: "These letters are a whole book; they're really fascinating." So, prompted by Tamm, I began to work on a book about my father, based on his correspondence.[3]

The second business association emerged in 1967 when I began collecting material about Paul Ehrenfest and learned that this great physicist and extremely interesting person held Tamm in high esteem. Tamm also told me lots of stories about Ehrenfest or relating to him, which I also used later on.[4]

And, lastly, at about the same time the Novosti Press Agency informed the staff of the Lebedev Institute that a British publishing house was planning a big series of books about Nobel Prize winners. The British publishers believed that books about

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[2] For more detail see Volkova I. M. "Odessa Group of Radio Specialists". In: *Academician L. I. Mandelstam: Birth Centenary*. Moscow, 1979, pp. 210—212 (in Russian).

[3] V. Y. Frenkel. *Yakov Ilyich Frenkel*. Moscow — Leningrad, 1966 (in Russian).

[4] V. Y. Frenkel. *Paul Ehrenfest*, Moscow, 1971 (in Russian).

Soviet scientists should be written by Soviet authors (the work was proposed to be done by B. M. Bolotovsky and myself).

By virtue of the above, I put down stories and episodes related to Tamm and Ehrenfest, this with Tamm's consent and in his presence. In his last years Tamm was compelled to speak more slowly. Many of his phrases (even those said in passing) stuck in my memory. Occasionally, I put them down word for word. I tried to reconstruct them on the following pages without reshuffling what I have heard from Tamm or sorting it out on the basis of the subject raised. I attempted to follow the sometimes quite unexpected zigzags to his thought, so characteristic of a lively conversation.

Our talks (before his operation in February 1968) usually started in the dining-room of his flat on the Maxim Gorky Embankment. Then we moved through to his study. The fittings were modest: a writing desk near the window, a popular photo of Leonid Mandelstam on the right, bookcases along the walls, and a round table beside the sofa. The table was laden in a natural but not irritating disorder with books which had been bought or presented to him recently (including detective stories in English which he was fond of reading), recent issues of physics journals and reprints.

The host seated his guest in the armchair or on the sofa, himself trotting or almost running the length of the room, making fairly frequent stops. His head was slightly inclined, his eyes glittering animately. A burning cigarette jutted out from the corner of his regular mouth. He spoke quickly, almost in a patter, so that the pauses he took to inhale smoke seemed relatively long.

Laughter tells about a man and Tamm laughed pleasantly, with all his mimicry and undertones. When he liked something in a story, he often interrupted the teller with a question and then raised his right thumb and said: "That's very good! I'll have to remember that."

Now a few words about his memory, particularly his professional memory. I have the impression that he did not remember his works well, especially his popular-science and philosophical writings. He said that each study engulfed him, that he was incapable of being distracted by any other work. It was as if everything else had been purged from his mind. Maybe that was why it took some time for him to recall his work on what are known today as the Tamm levels (it was in 1958, if my memory serves me right). He was genuinely surprised when I told him that it was considered a basic work and was referred to as such in many works on the physics of semiconductor (particularly transistors).

On some other occasion he recalled a funny episode related to

that work. It was in February 1932 in Leningrad (to judge by the final phrases of his article "On Possible Bound States of Electrons on the Surface of a Crystal"). It was when he worked at the Physical-Technical Institute that the idea occurred to him on the possibility of surface levels as a result of the "natural" imperfection of crystals — their finite dimensions.

"I do the calculations," Tamm said, "and get an absurdity. I do it again, and again I cannot find my mistake. One more time, the same result. Then I put aside all my previous calculations and start again. Again it's nonsense. Then I go to Yakov Frenkel.

" 'Yasha, I've got an algebraic kink, or something. Here, listen.' And I explain to him my idea.

" 'Very interesting.'

" 'Wait. And this is my calculation.'

"I write the formula and begin the transformations.

" 'As we know, a hyperbolic sine is an even function.'

Yakov Ilyich cuts in here:

" 'How do you know it, Igor?'

" 'Oh, damn it! It's okay now, Yasha. No more questions'."[5]

In 1961 Tamm went to the United States. I remember him telling about his visit to the Bell Telephone Company. Most probably he was reminded of a work he had done 30 years previously. When Tamm was still alive, a special book about that work of his was published abroad. The Russian translation came out in 1973.[6]

As to names and surnames, his memory often failed him. In a telegramme sent in the summer of 1960, he got a relative of mine's name wrong. Later he wrote a letter cursing his bad memory and asking to forgive him for the mistake.

Poetry, however, was his strong point. I heard him reading verse more than once. One episode in 1967 has stuck in my memory. By then Tamm had received many government and academic awards. The episode I'm referring to occurred at the Academy of Sciences' hospital in Moscow, where Tamm was undergoing a check-up — his doctors were worried about his blood analyses and feared the worst... His ward looked more like a hotel suite and was appropriately called "de luxe". Tamm sat in one of the armchairs, with his back to the window, and recited Boris Pasternak's poem *In Fame No Beauty*, doing it in a low voice and without affectation. The poem expressed the essence of his attitude to life:

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[5] Each fresh student knows that it is an odd function; the slip is an example of a sudden mental "blindness" — *Ed.*

[6] Davison S., Levin J. *Surface (Tamm) Levels.* Moscow, Mir Publishers, 1973 (in Russian).

for him selfless endeavour and devotion were the aim. He was indifferent to superficial success.

In several times heard Tamm recite Heine in German. But one occasion was especially striking. It all happened during his illness, i. e. after February 1968. We started talking about Alexey Tolstoy (a Russian poet of the 19th Century) and his poem *Arrogance*. The Tamms had a pre-Revolution edition of Tolstoy lacking some poems that Tamm wanted to re-read (*The Story of the State of Russia, Popov's Dream* etc.). I brought along a small volume of Tolstoy's works published in the "Little Series of The Poet's Library".

He was delighted to find *Popov's Dream* and began reading it at once. Then he wondered whether I was planning to stay in Moscow for long, for fear he might not have enough time to read the book. He was so agitated that I assured him that I would take it from him next time. Looking at his frame, I was not at all sure that I would find him sound and well in a month or two.

Fortunately, my fears proved unjustified. He was better, though not for long. Once again he was back to normal (if, of course, we forget his confinement to the iron lung). A few months later Tamm suddenly returned to the subject of Alexey Tolstoy. By that time he knew *Arrogance* by heart. And once again I heard him reciting a poem.

* * *

In 1965 Tamm was invited to Japan to attend a conference on the 30th anniversary of Yukawa's meson prediction. It was the more pleasant for him, because he specially liked his own 1934 work on the nuclear exchange forces, the point of departure for Yukawa.

In Japan the delegates were offered a choice between European- or Japanese-style hotels.

"Well, naturally, I chose a Japanese hotel, and regretted it very soon: national exotic features were abundant, conveniences and furniture were virtually non-existent."

The local services embarrassed him most. When Tamm, like any other guest, entered the hotel lobby, swarms of Japanese servants, trying to outbid one another in their readiness to do him some service, literally fell on their knees in front of him. Although, as he explained later, this was not an expression of servility, he was still extremely embarrassed.

It turned out that only 23 Japanese and 15 foreign scientists attended the conference.

"The papers were just crazy," Tamm said later. "But not in Bohr's meaning. I also read a crazy paper."[7]

I'll try to reproduce Tamm's commentary to one of the papers dealing with the classification of elementary particles. It offered a scheme based on three leptons. So what terms were to be used to describe a strong interaction? The responsibility was to be shouldered by "substance B", or a "paint," as Tamm called it. It was a "pra-baryon", so to say. I vouch for the accuracy of what follows.

"Just imagine," the speaker said, "a group of reticent people. They have come together and are now sitting quiet. Add some beer (Bier-Effect) and they immediately are chatting. An interaction emerges."

I remarked timidly that for all its anthropomorphism the image seemed witty and to the point.

"The trouble is that the paper contained nothing meaningful, except, perhaps, this analogy. What is worse, it was treated almost seriously!" objected Tamm. "I've been working like a galley slave for over two years. I've written thousands of pages. And what do you think is waiting for them? More likely than not the waste-paper basket." And he pointed vigorously to the one under his table.

Now that a two-volume edition of Tamm's works has come off the press, it is especially easy to find similar words in works where he was groping for the basic principles to a new theory. "My aim is to construct a theory free from divergencies. I'm well aware that my chances of achieving this are very slim" (1965). "For the last year and a half I have been slaving on an idea that caught me. The results have been varied" (1965). "I'm in my fifth year of labouring on this theory and there have been lots of ups and downs... Now I'm really optimistic" (1968).[8] The latter phrase is particularly significant because it was written when Tamm was bound to an iron lung.

Leafing through my 1967 diary I see an entry "April 27, 16.30: dinner at the Tamms". I remember that day well. When I arrived, Tamm was out. Natalya Vasilievna explained that lately he had seldom taken walks and they had even had to think up some errand to get him into the fresh air. Then Tamm showed up. He looked

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[7] I. E. Tamm was very fond of Bohr's remark that a truly novel theory must always seem crazy at first. He wound up his talk ("On a Curved Momentum Space") in Japan with the following words: "Thank you for your attention to such problematic and crazy ideas."

[8] I. E. Tamm. *Collected Scientific Works*. Vol. 2, Moscow, 1975, pp. 218, 463 and 486 respectively (in Russian).

a bit hunched compared with what he had been like when I had last seen him some four months earlier. But when he sat down it was not so conspicuous, and he looked like the man I had met in previous years. The only thing I was not accustomed to was seeing him so static. Normally he leapt up suddenly to pace the room in his characteristic manner, hands either behind back (when listening) or gesticulating vigorously (when saying something).

Tamm looked with interest through the Xerox copies of his letters to Ehrenfest that I had received from Leiden.

"Well, I don't remember a thing! But then I don't even remember all of my own works. I always forgot all about them as soon as some new idea came along."

He told me about a lovely bike tour he had made of Holland in 1928 with the Shubnikovs (Lev Vasilievich and Olga Nikolayevna),[9] and Ehrenfest's elder daughter Tatyana.

"Nowhere have I used a bicycle as much as in Holland! Incidentally I taught Dirac to cycle and he nearly fell into the canal in the process."

Some time later I added some details I learned from Olga Nikolayevna to his reminiscences. For a long time Ehrenfest had refused to allow his daughter to join the Shubnikovs' cycling party, but when he learned Tamm was going, he relented: Tamm was the oldest of them all at 32 (6 years older that the Shubnikovs and 10 years older than Tatyana Ehrenfest).

I soon began preparing correspondence between Ehrenfest and Ioffe for publication. Reading Ehrenfest's letters of 1928, I found a lot of material (part of it still unpublished) related to Tamm. I put it down and brought it along to show him (that was somewhere around 1969). On April 13, 1928 Ehrenfest wrote: "To my delight, T' ('Tanya-prime' was what the Ehrenfests called their elder daughter — *V. F.*) has joined Tamm and the Shubnikovs on a cycling tour to the picturesque parts of south-east Holland for the Easter holidays. She sends exalted postcards home. And it is all thanks to Tamm's energy. He has even turned the whole group into regular tennis players."[10]

And one more touch to finish the story. When Tamm passed on, his family lent a few photographs for an article in the *Khimiya i zhizn* (Chemistry and Life) magazine. One of those amateurish

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[9] The married couple L. V. Shubnikov and O. N. Trapeznikova, both experimental physicists and later professors, were on a long trip to Holland on scientific business. At V. de Haas's laboratory in Leiden the Shubnikov-de Haas Effect was discovered — *Ed.*

[10] *Ehrenfest — Ioffe. Scientific Correspondence.* Ed. by V. Y. Frenkel. Moscow, 1973, p. 204 (in Russian).

photos (probably taken by L. V. Shubnikov) showed Tamm, Olga Shubnikova and Tatyana Ehrenfest. Tamm, still a youth, stands by the road-side, hands in pockets. A vest shows through the unbuttoned overcoat. The three are standing there with a young pine grove in the background, as if it were some place near Moscow, not Holland.

Here are two more excerpts from Ehrenfest's letters to Ioffe. The first one is from the letter of April 13 quoted above: "I find Dirac's latest work on electron spin just splendid. Tamm has explained it all to us very well. He is continuing to work on this. Tamm is glad that Dirac will spend May and June at Leiden. The Lorentz Foundation has invited Tamm to extend his stay at Leiden for at least that period (it would be a good idea to say this in print somewhere in Russia!). So Tamm will be a guest of the Lorentz Foundation from mid-January till the end of June. He is only the second man to be invited by the Lorentz Foundation for such a long period. The first was O. Klein from Copenhagen, one of Bohr's associates. The Lorentz Foundation trustees include Zeeman, myself and Fokker. At the time of Tamm's invitation Lorentz himself was on the board."[11]

The second excerpt is from a letter of March 5, 1928: "If you really want a young theoretical physicist in charge of genuinely lively, clear-cut and critical teaching some place, get Tamm, giving him a compact but good library."[12]

Tamm always smiled pleasantly when he remembered Ehrenfest. He believed Ehrenfest's part in developing the new physics to have been underestimated, personally placing him in the same flight as Mandelstam. In his lifetime, Ehrenfest's sharply critical brain somehow obscured the significance of his own positive research into statistical mechanics, relativity and quantum theory.

"True," Tamm said, "he had no match when new works were being discussed and their physical essence fathomed. Take Dau (L. D. Landau — *Ed.*), for instance. Criticising a specific theory was just his cup of tea. Mind you, if he wrote off one of my more general ideas in his categorical way, by his characteristic 'It's nonsense!' I thought that the question was worth pondering on."

Tamm would often talk about Dirac. They first met at Leiden in May 1928. Ehrenfest asked his assistants to meet Dirac at the railway terminal. Tamm joined the welcoming party. None of them knew Dirac personally, therefore everyone took along a reprint of Dirac's latest work on relativistic electron (the one reviewed by

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[11] *Ehrenfest — Ioffe. Scientific Correspondence.* Op. cit., p. 207.

[12] *Ibid.*, p. 203.

Tamm a month earlier at Ehrenfest's theoretical seminar at Leiden) and positioned themselves at the exit of every carriage. Dirac took the "bite".

Dirac and Tamm soon became close friends, and Ehrenfest was delighted. On June 2, 1928 he wrote to Ioffe: "Tamm and Dirac have become very friendly. In mid-June they will go to Leipzig together where Debye and Heisenberg are organising a week-long discussion on quantum mechanics." For Ehrenfest there was no greater pleasure than seeing good, comradely relations developing between two people whom he respected.

Here is another story Tamm liked to tell about Dirac. During one of Dirac's visits to Copenhagen, Bohr came to see him one evening. That same day Dirac had received the proofs of his article in the *Royal Society Proceedings*. (Tamm did not specify the article, so it is difficult to date this meeting.) Bohr, who used to improve his proofs ten times over, read the article and asked:

"Dirac, why have you only corrected few misprints, and added nothing new to the text? So much time has passed since you wrote it! Haven't you had any new ideas since then?"

Dirac answered unruffled:

"My mother used to say: think first, then write."

A Leningrad newspaper (Y. I. Perelman ran its science section) published an article about physicists coming to the First All-Union Conference on Nuclear Physics (September 1933) suggested by A. F. Ioffe, I. V. Kurchatov and other scientists from the Leningrad Physical-Technical Institute. Dirac arrived by sea. In an interview at the harbour he said that Tamm was among the Soviet theorists, whose works interested him most.[13] Dirac's letters to Tamm were extremely friendly.

Our conversation moved naturally from Dirac to the modesty Tamm so valued in people, and he thought right away of Mandelstam. As I was interested in Ehrenfest he told me the following episode which occurred in September 1924 in Leningrad at the 4th Congress of Russian Physicists.

At the Physics Auditorium of the Polytechnical Institute a paper was read on diffraction. Perhaps Ehrenfest was telling of the "Quantum Theory of Fraunhofer Diffraction" work which he had performed with P. S. Epstein that year. Tamm was sitting next to Mandelstam somewhere about the 10th row in the stepped lecture hall. The paper provoked debate and at some stage in the proceedings Ehrenfest broke in and said:

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[13] *Vechernyaya krasnaya gazeta* (Leningrad), September 28, 1933.

"And now perhaps the world's greatest authority on optics, Professor Mandelstam will say something."

And he began to look round the auditorium. Mandelstam became frightfully embarrassed and, to Tamm's amazement, literally crept down onto the bench so that he was not visible from where Ehrenfest stood.

I started scribbling it all down on paper but Igor Evgenievich remarked:

"Perhaps it would just not be the done thing to write 'crept down' straight off?"

"Then let's write that he hid behind you."

"Well, I suppose we might."

Three years later the book on Ehrenfest came out and this incident surprised one of the readers who asked how the well-built Mandelstam could possibly have hid behind the short Tamm.

I muttered something incoherent and told Tamm what had happened the next time I saw him.

"Indeed we might have done better to write it all just as it was. Leonid Isaakovich's reputation would not have suffered."

Igor Evgenievich took great pleasure in talking of Mandelstam.

Tamm was very impressed by A. N. Krylov's speech at the gathering on December 22, 1944 to pay last tributes to Mandelstam. It was not quite Krylov's usual style, but his literary talent stood out.[14]

I'm not sure whether or not Tamm knew that Krylov had vigorously supported his candidacy at the 1943 elections to the Academy. This support was expressed in letters to A. F. Ioffe (then Vice-President of the USSR Academy of Sciences and Secretary-Academician of the Department of Physics and Mathematics) and P. L. Kapitza. Krylov was very serious about his representation, and this is borne out by several rough copies in which he polished his style and refined his arguments, emphasising Tamm's research into the Cherenkov effect theory.[15] Krylov said he had understood the meaning of the work after discussing it several times with Mandelstam.

In early 1967 I selected some photos I had at home which I needed for an article I was writing. Naturally, I was looking for snaps that had not been published before. But on some of the pictures which my father took between 1926 and 1931, I saw

[14] He ended his speech with the words: "Let the soil be soft for him, for he was a righteous man" (A. N. Krylov. *Reminiscences and Essays*. Moscow, USSR Academy of Sciences Publishing House, 1956, p. 503 (in Russian).

[15] Archives of the USSR Academy of Sciences, f. 759, item 2, pp. 3, 4 (in Russian).

some unfamiliar faces. I showed them to Tamm, but he could not recognise anyone either. "I can't remember faces," he explained. But then he flashed a wide and joyous grin: he had seen a group photo with Niels Bohr.

"A very characteristic turn of the head!" Tamm exclaimed.

The next episode also dates to 1967. I told Tamm about the splendid Boltzmann biography written by the Austrian physicist E. Broda, and that Boltzmann's works had come out in Russian translation.[16] I felt they might be of interest to Tamm in view of his own latest works.

"What do you think about Boltzmann's (and Poincare's) idea of time discreteness?"

"Such judgements should not be taken too seriously because they are close to unessential (from the level of knowledge at the time) conjecture and supposition. As a matter of fact," Tamm added, "that holds good for Democritus' atomistic views".

He believed that the atomism of Greek philosophers was just a curio.

We then got on Einstein's works which had just come off the press (Tamm headed the group of editors). I remember Tamm being very pleased with Smorodinsky's contribution.

"Have you met Einstein?"

It turned out that he had only met him once and very briefly at Ehrenfest's home at Leiden. Unfortunately, Tamm did not remember the details. Yet he well remembered a story he heard from the pianist G. Neuhaus, who himself had heard it from Leopold Godovsky, his teacher, who knew Einstein. According to the pianist, Einstein, continuously eulogised at a dinner party, leaned to Godovsky and said in a low voice:

"I get the feeling I've done something wrong."

"Probably, he was afraid that people might think he was seeking popularity," Tamm explained.

"The fourth volume of Einstein's collected works is full of phrases which can be interpreted (given the will or careless reading) as evidence of religiosity," I continued.

"Of course, Einstein was not a religious man, and his own remarks on the score should not be taken literally. Those were just metaphors. I know some scientists who are religious 'in a regular way'. They are worthy enough men, but I can't understand them!"

Tamm's impression was that Western intellectuals often seek ideals common for both sides of the political divide. Such an ideal

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[16] E. Broda. *Ludwig Boltzmann*. Berlin, 1957.

is universal peace (the Pugwash Movement is an expression of this quest). Attempts to create a new religion free of mysteries and fables show that many people in the West hope to improve thereby mutual understanding and lessen the danger of war. Tamm used to say that the main contradiction of our time was the conflict between old instincts and new technology. Would people ever learn to control their instincts? In the past, these unbridled instincts did not entail such fatal consequences (indeed, how many people can one kill with an axe or a knife? Twenty or thirty?), whereas now millions upon millions can become victims of malicious intent or misadventure.

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In October 1967 I brought Tamm I. N. Golovin's book[17] about I. V. Kurchatov. He immediately began to leaf through it, pausing at the photographs.

"I read somewhere that in memoirs and books about scientists they utter only aphorisms and this takes them away from truth. Maybe the explanation is merely that such things are remembered longer."

Tamm and Kurchatov met in Leningrad. Tamm visited his laboratory at a time when Kurchatov was studying ferroelectricity and was just toying with the idea of switching to nuclear physics. Then came their meeting at the First Conference on Nuclear Physics in 1933, following which they met regularly at every such conference.

Tamm recalled that in long gone times he and Mandelstam discussed the big part the organisers of research played in science. He singled out Kurchatov (with whom he had worked in the 1950s) and the biologist V. Y. Gavrilov among the scientists endowed with organising talents.

Passing over to another subject, he said that the dramatic growth in the importance of science in the life of society also had certain disadvantages. "It wouldn't be bad," he remarked jokingly, "if science workers' salaries were trimmed a bit. So many people who don't belong are worming their way into science nowadays. Really, it looks as if it's far less profitable to be a highway-robber."

It was the afternoon. Tamm suggested that we go out and have a breath of fresh air. We took the lift and went down to the

[17] I. N. Golovin. *I. V. Kurchatov*. Moscow, Atomizdat, 1967 (in Russian).

courtyard of his house on the Gorky Embankment. Then we skirted the tall building and came up to two benches in a small public garden, standing back to back. One faced the facade of the house, the other, smaller, was turned to the Moscow River. Tamm said, taking his seat:

"This one suits my height better. And the view is much lovelier."

He gasped for breath for a while and then said:

"I have pulmonary emphysema. In the USA, it kills more people than TB does. But cancer, of course, is the biggest killer. How come? Most probably several factors are to blame. Man's average life span has increased. In the past people simply didn't have time to die of cancer. Medicine has made remarkable progress in controlling infectious diseases such as TB. Correct diagnoses probably also play a part."

Tamm then said that American postwar statistics put the mortality rate from cancer as rising with the tobacco consumption curve (or the number of smokers). Hence the conclusion of a correlation. But Professor Mahalanobis (Tamm met him in 1962 in Calcutta) suggested that those figures needed rechecking. It turned out that the sharp increase in smokers was due to women taking up the habit in a big way, while the mortality rate among them had remained practically unchanged. The number of men-smokers, however, had not changed, while their mortality rate had increased. One of the possible explanations was that men spend most of their time at work in town, where cancerogenic substances are abundant. "There are so many misunderstandings and pseudo-discoveries caused by an inability to use statistical data and instrument readings correctly", Tamm said. "Pauling made me a gift, a book entitled *How to Lie with Statistics*. It's a pity someone has helped himself to the book. It might have been of interest to you."

I recall Tamm saying at that time that it was wrong and unjust to divide the professions into prestigious and second-rate. He was especially indignant at haughtiness and slight in respect of occupations that narrow-minded people regard as second-rate. He recalled, in particular, an extremely interesting conversation he had with a driver in the Pamirs who was an astrobotany enthusiast. No less interesting were his get-togethers with workers on Kamchatka whom he read lectures on Uranium. Tamm was delighted with their interest and knowledge. And, passing over from professions to sciences in general, he summed up aphoristically: "There are no second-rate sciences, only second-rate scientists."

* * *

I recall going to see him on December 28, 1969. I told Igor Evgenievich a lot of new and worn jokes and heard several that came back to him. We began talking about jokes and how they were all mini-tragedies in their own way ("A looney...", "My mother-in-law is dying...", "A husband catches his wife...", "Someone falls from the 7th floor..." etc.). We long racked our brains to find an exception and came at length to the conclusion that, even if no one died or was injured, the heart of the joke was nonetheless more sad than amusing.

"Often," remarked Igor Evgenievich, "the behaviour of people in a given situation is funny only at first sight. If you really think about it, you will have sad sentiments. For instance, I knew a young physicist back before the War. He came to visit me and went straight from our place to the railway station thinking he would buy a ticket at the last minute. At 1 a. m. we got a knock at the door and there the fellow stood. He explained his plight by 'I only travel first-class and there was no vacant place.' The laugh of it all was that all the beds were taken up and he had to sleep on the floor. Ridiculous vanity, arrogance, haughtiness? But then that's lamentable."

* * *

My memory holds episodes outwardly unlinked just as in a movie film there is rarely continuous action for more than 5 minutes. Certain associations clear to one or perhaps even both of us took the conversation onto an off-shoot along which we went so far that there was no return. Maybe an exchange of jokes on the mad was what led Igor Evgenievich to talk about the crazy letters he would get, and in large numbers. There were plenty of different themes touched upon in those letters, but all of them had the same aggressiveness and all-mightiness.

"A feverish physical pattern is applied to the pseudo-logical canvas. I recently received a letter," said Tamm, "speaking of the love of Christ for more than people, even for the entire inorganic world. This seemed to strike a chord with the author. But he finished off with: "I simply cannot grasp how the proton can love the electron with them being so different in mass!" Igor Evgenievich recalled how he had once replied to such a letter trying to get the author to his senses, to show him that his argument came from non-science. Soon a second letter came: "We'll get even with you!"

* * *

Physicists of Tamm's generation lived through a scientific and a social revolution. Tamm's contribution in physics is well known. But many younger physicists, not to mention students who learn the fundamentals of electromagnetic field theory from Tamm's book, will hardly suspect that he was involved in the tempestuous revolutionary events of 1917. It is a fact, however, that Tamm was a delegate to the First Congress of Soviets in Petrograd and was elected to the Executive Committee of the Soviet of Workers' and Soldiers' Deputies in Elisavetgrad.

In the stormy years of the Civil War (1918—22), he several times found himself in situations when his very life was endangered. His adventures would make an exciting story. I heard about some of them from Tamm himself. They were not the long sagas told around the camp-fire, but small stories that would surface in his memory spontaneously, when he digressed from the subject of the conversation. Here is one of them.

In the autumn of 1919 Tamm was in Kiev which was occupied by the Whites. Since he had no identification papers (the only papers he had having been issued by Soviet authorities), he was likely to be arrested or at best taken for the army. So he decided to get to the Crimea, to Tavria University. He was given a recommendation by a Kiev physicist he knew. With great difficulty he managed to board a train for Odessa. As people might recognise him in Elisavetgrad, he decided to stay inside the carriage until the train had passed his native town. In the evening the train stopped at Novoukrainka. It was not going any further because some of the trains ahead of them had been derailed by Makhno bandits.

Fearing a possible round-up at the station, Tamm went to find lodging for the night in the small town a mile from the station. In the morning, hunger drove him to an eating place pretentiously called a "restaurant". He had just ordered a cup of coffee when a military patrol came in and began checking identity papers. Luckily at that moment two officers came to the restaurant. He recognised the junior officer whom he had met on the train.

He was a Greek called up to the French army. His name was George. For some complicated family reasons he had found himself in Civil War-torn Russia, and was now making his way to Odessa. Monsieur George saw Tamm and introduced him to his companion — a White-Guards colonel. George knew that Tamm had no papers and quickly assessed the situation. "Well," he said, "I don't think we will get much of a breakfast here. Colonel, let's go and find somewhere more decent."

The threesome headed for the exit, with Tamm bringing up the rear. His appearance, however, aroused suspicion with the soldier standing at the exit. "Your papers!" he demanded. But the colonel cut in: "Let him through. This gentleman is with me."

They looked for a better place but in vain. The colonel then invited George and Tamm to his place, and they went back to the railway terminal. The colonel had a personal carriage at his disposal.

"It looked like an ordinary freight car of 1918," Tamm recalled. "But inside it was like a well-furnished living room. We had hardly made ourselves comfortable around a small burning fire-stove and the colonel put some food onto it when somebody knocked at the door: 'Open up!' It was a new patrol. The colonel opened the door slightly and said no, there wasn't anybody else in the carriage. He closed the door and, turning to me, he said: 'Well, I don't even know who you are. You do have a passport?'" Tamm paused here, and said with a smile:

"What is a man? A body, a soul and... (he paused again and raised his forefinger) and a passport!"

Tamm reached Odessa safely, boarded a ship to Sevastopol and in the late autumn of 1919 was given a job: he began working as a research fellow at the Physics Department of Tavria University in Simferopol. But very soon his political views and attitudes became known and by the summer of 1920 he was forced to flee from Simferopol. Many years later he was told that just a few hours after he had left his room the police came to arrest him.

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Tamm worked in the forefront of theoretical physics at a time when the general principles of quantum mechanics were being developed and applied to specific physical phenomena. It was the kind of time when several scientists might arrive, simultaneously and independently, at similar ideas and develop similar theories. An instrument to measure such coincidences would be overloaded. A scientist's behaviour in such who-was-first situations is a touch-stone for appraising his human value. Tamm's behaviour was impeccable. It was an example to be followed and an ideal hard to attain. That he was scrupulous and correct in the highest degree shows through clearly in his printed works.

Historians of science do not untangle the "priority knots" out of mere curiosity. The truth they are after is all the more elusive because such "knots" tend to have psychological "threads"

where the scientific and the personal intertwine. Coincidences (which demonstrate the law-governed nature of the general course of development in a specific branch of science) exist alongside ill-intentioned acts where generally accepted moral principles have been undermined.

I remember Tamm being indignant at G. A. Gamov for not mentioning in his well-known work on the theory of α-decay, the paper by L. I. Mandelstam and M. A. Leontovich in which it had been demonstrated in detail for the first time that in quantum mechanics a particle can go through a potential barrier, which is exactly what is called the tunneling effect.[18] It was the basis of the Gamov theory. Tamm said, perplexed:

"How come? He is a talented man, his works are good, and I'm certain he knows Mandelstam's and Leontovich's article. Well, perhaps brains or no brains is not the problem here, but rather an other department..."

I asked him "priority questions" concerning works by Y. I. Frenkel too, since in his article in Frenkel's remembrance (1962) Tamm himself had touched upon the subject, if only in passing. He was grieved to hear of one of my father's colleagues intentionally passing over his works in silence. I recall another instance. In the early 1960s we talked about a prominent scientist whose moral standards Tamm commended. I had good reason to believe otherwise (at least in the past) and said so.

"That is impossible!" Tamm exclaimed.

With hindsight I'm inclined to regret how I acted further. In Moscow again some time later, I brought Tamm a document I had found in our family archives, which corroborated my judgement. Tamm read it, looked sad and shook his head. There was an uneasy pause. I swore to myself never again to engage in that sort of "enlightenment". A Man with a capital "M" exerts a powerful influence on other people, who within limits may change for the better with each passing year. People revealed their best qualities when dealing with Tamm. They did it quite unconsciously fascinated by his personality, and were not motivated by long-range career objectives. This happened to many people who met him.

When Tamm saw narrow-mindedness, stupidity or unseemly behaviour, he recalled his dear colleagues and friends, as if seeking to find refreshment in such memories. He often spoke of the Theoretical Physics Department he headed at the

[18] For more detail see: I. E. Tamm. *Collected Scientific Works*. Vol. 2, Moscow, 1975, pp. 382, 459 (in Russian).

Physical Institute. I remember some of his phrases word for word.

"I find happiness in my colleagues. They are all gifted people, and some of them are big talents. So many years have passed and there was never a row or squabble."

He was never stinting in praise when one of his close associates or friends was mentioned. At such moments he flashed a broad smile. Sometimes he even closed his eyes a bit and said:

"Oh, he is very clever! He's a favourite of mine!"

As regards most regrettable priority faux pas, Tamm was perhaps most critical in respect of works on the combinational scattering of light (which was, incidentally, the subject of several articles of his). He firmly believed that the Nobel Prize Committee was flagrantly unfair in deciding to honour C. V. Raman alone. Tamm stressed that the research done by Mandelstam and Landsberg practically simultaneously with Raman and absolutely independently was more profound and, beyond merely describing the effect, interpreted it correctly. Tamm approved of Max Born's decision to leave the Nobel Committee because of the position it had taken.

Tamm also believed the Nobel Committee not bold or principled enough when it awarded the Prize to Einstein not for his main work, the theory of relativity. He recalled that recently he proposed E. K. Zavoysky be given the Nobel Prize for his discovery of electronic paramagnetic resonance and thought it a big mistake that the Nobel Committee overlooked Zavoysky's outstanding work (performed interestingly during the war years when Tamm and Zavoysky were both at Kazan University, and reported at a seminar organised by the Lebedev Institute).

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In the early 1957 A. F. Ioffe invited Tamm to a seminar sponsored by the Institute of Semiconductors to commemorate my father. The seminar was held in a building on the Petrovskaya Embankment which also housed the Institute of Brain and the Institute of Lake Studies. Some thirty or forty people assembled in a small room, part of a hall partitioned off by plywood walls, and with a still richly painted ceiling. Tamm gave a general assessment of Y. I. Frenkel's works. Ioffe, who spoke next, laid special emphasis on their influence upon experimental physics. The mood was extremely cordial and friendly. Tamm was as relaxed as if in conversation.

It would probably be appropriate now to say a few words about Tamm and my father. Tamm's personality was especially vivid

when he met friends, pupils and associates. When I think about it, I recall one of Tamm's little stories about Dirac. Once, after Dirac had read a paper during a visit to our country in the late 1950s, he was asked about the inner composition of particles. In reply he said something like this:

"Does it really matter what material was used to make chess pieces? What is important is what laws govern their interaction on the 64 squares of the board."

Rich material for research is indeed provided by Tamm's relations with his contemporaries, such as Bohr, Dirac, Ehrenfest, Vavilov, Kapitza, Landau, Mandelstam and Frenkel; by the influence exerted upon him by his predecessors, such as Faraday and Helmholtz; by Tamm's traditions in the Theoretical Department of the Lebedev Institute; and by Tamm's style as seen in the work of his followers.

Tamm and my father were about the same age and in some respects even looked alike: they were both short and very agile. True, Tamm was lean and athletic, whereas my father was a bit on the plump side. Yet they were close primarily because of their profession, although their immediate interest did not always coincide. They shared views and fought together for a new physics, finding themselves in the same camp when they became targets for those primitive philosophers who denied the importance of quantum mechanics and relativity condemning them as bearers of pernicious idealism. (This camp included theorists such as V. A. Fock and L. D. Landau, and experimentalists such as A. F. Ioffe and S. I. Vavilov.) The principles underlying Frenkel's and Tamm's textbooks on electrodynamics differed sharply, yet both praised each other's books. In general, their close friendship did not mean that their views necessarily coincided. There were always matters on which they disagreed.

They often discussed each other's work (this is evident from the conclusions of their articles and from references), although there were only two occasions when they were the co-authors of articles. Those were the articles which criticised N. P. Kasterin's and A. K. Timiryazev's erroneous views on electrodynamics.

Their assessment of their colleagues' work was different. My father was more lenient. If, for instance, a dissertation he was invited to be an opponent to was not brilliant and needed substantial rewriting but was written by a man with good record in serious research, my father believed it possible to give it a positive assessment. Tamm, on the contrary, evaluated it for what it was, and remained inexorable. Still a boy in Kazan, I remember my father urging Tamm to be more tolerant and reminding him of some circumstances not relating to the dissertation itself, but

personal. He always came up against Tamm's vigorous opposition. So my father disagreed with Tamm while respecting his point of view.

There were some funny incidents. When he visited the United States in 1931, my father published an article in *Physical Review* on the theory of the photoelectric effect. In it he gave a wrong account (someone had misled him) of one of Tamm's works. Tamm refuted that part of the article in his own piece in the same magazine. A few years ago (after Tamm's death) the late Professor Ed. Hill, my father's assistant at the Theoretical Physics Faculty at the University of Minneapolis, sent me the originals of letters my father had written him. In his letter from Leningrad (dated February 18, 1932), he wrote:

"Now I'm interested in the quantum theory of the surface properties of solids, working in this field with my friend Tamm who is here a month. (I hope you don't think that we've quarrelled because of my unfounded criticism of his work on the photoeffect, which I hadn't read at the time, and his sharp reply, which was fully justified.)."[19]

I think there were occasions when Tamm erred in his assessment of my father's works too. Such was the case with the theory of electronic spin (1925—1926). In 1958, however, Tamm reconsidered the importance of this work (as against what he had written about it in 1932) in a special article about my father.[20] None of this ever marred their relations whose closeness was amplified by family friendship; my mother loved Natalia and Igor Tamm, while Natalia and Igor loved (I take the liberty of saying so) my parents. The chance to meet Tamm and to enjoy his kindheartedness is a precious part of the inheritance I received from my parents.

* * *

Once I asked Tamm about the origin of his interest in biology. He said that when he was still at school in Elizavetgrad (now Kirovograd) he made friends with B. M. Zavadovsky, a boy in his class who was to become a prominent scientist, a Member of the Academy of Agricultural Sciences, and had specialised in the physiology of the endocrine glands. At school Zavadovsky

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[19] For more detail concerning the relations between I. E. Tamm and Y. I. Frenkel see I. L. Fabelinsky's reminiscences in this collection — *Ed.*

[20] Tamm I. E., Smorodinsky Ya. A. in: Y. I. Frenkel. *Collected Works*. Vol. 2, Moscow, Leningrad, 1958, pp. 455—460 (in Russian).

had liked biology and spoke about it with enthusiasm.[21] In the summer of 1914, Tamm read Helmholtz's *Mechanics* which consolidated his wish to become a physicist. But he also read some of Helmholtz's books on other subjects. He was particularly impressed by the German scientist's studies of the physics of sound and the physiology of auditory sensation.

As the conversation went on, Tamm "leapt" into the 1950s. He renewed his interest in biology, he said, because of some fundamentally new discoveries and the unsatisfactory progress of this science in the Soviet Union. He began meeting many biologists, such as B. L. Astaurov, N. V. Timofeyev-Ressovsky and V. A. Engelgardt.

Many years earlier, Tamm was undoubtedly influenced to a considerable extent by biologists from Tavria (now Simferopol) University, where he worked from 1919 to 1920. They included Professor A. G. Gurvich[22] and his assistant A. A. Lyubishchev, who were both also keen followers of physics and mathematics.

Between the 1920s and the 1960s Lyubishchev's works were known only to biologists and entomologists, though now (after his death in 1972) they feature in many writings. Lyubishchev's impressive personality both as scientist and man attracts many authors, physicists and mathematicians.[23]

Gurvich played an important part in Tamm's scientific life. It was he, in 1920, who gave Tamm a letter of recommendation to Mandelstam. Tamm recalled: "I came to him from Simferopol with Gurvich's letter, and he immediately began to work with me. We became friends and thereafter he was my 'patron'." Mandelstam's opinion in complicated situations was always of extreme importance to Tamm.

Gurvich's name and works were especially popular in the 1920s and 1930s, though today his studies are familiar to a relatively small circle of specialists. In late 1964 Natalia Tamm told me that after a long break her husband had received a letter

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[21] B. M. Zavadovsky wrote several popular books on biology (see *Great Soviet Encyclopaedia*, 3rd edition).

[22] For more detail about A. G. Gurvich see: L. V. Belousov, A. A. Gurvich, S. Y. Zalkind, N. N. Kannegisser. *Alexander Gavrilovich Gurvich*. Moscow, 1970 (in Russian).

[23] For more detail about A. A. Lyubishchev see: S. V. Meyen, B. S. Sokolov, Y. A. Shreider. "Classic and Non-Classic Biology. The Lyubishchev Phenomenon". In: *Vestnik AN SSSR*, 1977, No. 10, pp. 112—124; Y. A. Shreider. "The Three Lives of Professor Lyubishchev". In: *Roads into the Unknown*. Collection No. 14, Moscow, Sovetsky Pisatel Publishers, 1978, pp. 393—397 (all in Russian).

from Lyubishchev. I had heard about him from my parents and even remembered his coming to Leningrad in the late 1940s. But then I lost track, and now it turned out that all those years he was working with good purpose in Ulyanovsk.

"He is a very interesting person with a brilliant memory," Tamm remarked. "He can tell you a lot about Tavria University." Tamm then recalled that in those days in the Crimea Lyubishchev was interested in the symmetry of the organic world and searched for ways to describe it mathematically. He regularly attended the sittings of a mathematical society headed by N. M. Krylov, and the meetings of the Physics Department, where Frenkel and Tamm read lectures on the new physics. Tamm recalled that Lyubishchev once asked him to construe a passage in a German monograph on mathematics that he could not understand. The problem turned out to be extremely complicated. Tamm racked his brains over it but could not figure it out. Some five years later, in 1924 or 1925, when he was already in Moscow, he happened to come across a Russian translation of the book. Tamm immediately remembered his talk with Lyubishchev and found the passage. As it turned out, the editor had included a thorough commentary and shown a major error by the author.

When in the mid-1960s I met Lyubishchev in Leningrad, he remembered the book and the passage with the mistake. Lyubishchev was pleased to hear that Tamm remembered the episode. He exclaimed:

"I'll certainly get hold of that Russian translation!" I wonder if he realised his intention.[24]

I am convinced that Tamm's short stay at Tavria University influenced him as much as it did my father who worked in the Crimea longer. It was there that they first met, became friends and made the acquaintance of major scientists and remarkable people such as literary critic N. K. Gudzii, A. G. Gurvich, A. A. Lyubishchev, and the mathematicians N. M. Krylov, V. I. Smirnov and M. L. Frank. I might say that Tamm's and Frenkel's names, together with the names of the other founders of Simferopol University, are engraved on a marble plaque on its wall. I remember the interest with which Tamm read a collection of articles put out to mark the 40th anniversary of the University and sent him as a present.[25]

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[24] Professor Barantsev told me that the book in question was *The Elementary Textbook of Algebraic Analysis* by E. Cesaro, which was the subject of Tamm's and Lyubishchev's letters.

[25] See: F. S. Zagorodskikh, et al. *The History of the Crimean Teacher Training College.* Simferopol, 1960 (in Russian).

But all this I learned much later. In October 1957 Tamm's intention to read a paper on the molecular mechanism of heredity surprised me, and, for that matter, the bulk of physicists and mathematicians in Leningrad. All of them, plus an army of physics, biology and maths students, packed the History Faculty lecture-hall at Leningrad University, overflowing onto the stairs and entrance hall. The lecture was part of a series on genetics read at the University and at the USSR Academy's Botany Institute.

More than 20 years have since passed and I have only a dim recollection of Tamm's paper. But many of those who heard it remember that Tamm was warmly introduced by P. A. Baranov, a Corresponding Member of the Academy of Sciences, who had known him since their meeting in the Pamirs. Tamm began his lecture approximately as follows:

"I see that the audience today is predominantly young. Many years ago when I had to decide who to be, I was absolutely sure there was nothing more interesting than physics. Today if I was to choose I'm not so sure what I would actually decide. It seems that now the future is with biology."[26]

Tamm told the audience about Gamov's work and the research done by James Watson and Francis Crick. Many people heard the strange words "DNA", "RNA" and "double helix", now so commonplace, for the first time that day. He illustrated the idea of a genetic code with examples from combinatorics. I don't remember all the details of Tamm's lecture, but L. I. Vernsky managed to find a verbatim report, so I can cite a few excerpts.

"... I would like to draw a distinct line between what I've said so far and what I intend to say now. So far, all I have said remains correct and verified; all I shall say now is unknown, uncertain and does not correspond to reality. I shall speak about that not because it offers a concrete solution, but because the way the objective is set is very important, as is the fundamental possibility of formulating answers... Well, what is the idea of encoding information?"[27]

Tamm then spoke of the unsuccessful attempts by Feynman and Teller to construct a code. "It'll take me five more minutes to finish up... I'll tell you about a very interesting code Crick has invented..." When Tamm finished his lecture, those present

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[26] Tamm published an article in the magazine *Tekhnika — Molodyozhi* (1957, No. 9), which he significantly entitled: "In a Relatively not So Distant Future Biology Will Take the Place of Physics as the Leading Natural Science".

[27] See: The Verbatim Report of I. E. Tamm's lecture "Physics and Genetics", read on October 23, 1957 at Leningrad State University, p. 13 (in Russian).

began, slowly at first, sending him notes with questions. As soon as Tamm received the first question, he jumped off the dais where the rostrum stood, although he could have used some steps off the stage. He did it naturally, with ease. He was 62.

* * *

From the mid-1960s, my visits to Moscow became more frequent, while Tamm's mobility substantially declined. Earlier when I caught him in Moscow, I thought myself lucky; now, however, the chance of missing him was, alas, drastically diminishing with each visit. The last five years of his life Tamm practically did not leave Moscow, except on rare visits to his cottage. In February 1968 we learned about his operation. It had fatally limited his mobility. The love his colleagues and his many friends felt for him became more obvious, as they watched him with great concern and hope wrestle with the crisis. His awareness of this, conscious or subconscious, may have given him strength and helped him physically, and even more so psychologically, combat his illness. On seeing Tamm at home after the operation, visitors were immediately affected and greatly impressed by his fortitude at this phase of his illness. All outward signs receded into the background. No one could help admiring his courage, optimism, his powerful intellect and thirst for life.

The news of Tamm's death shook everybody though it was not unexpected. On April 15, 1971 hundreds of people attended the civic funeral held in the Assembly Hall at the Lebedev Institute. When they came out onto Leninsky Prospect, they saw bright sunshine flooding the street: spring had come to Moscow... A line of cars several blocks long proceeded to the Novodevichye Cemetery. Tamm's closest friends and associates bore the coffin to a small place near the fence, where a funeral ceremony was held. I. M. Frank said in his funeral speech what everybody had on his mind: "When Tamm fell ill, we all learned how great was the courage of this remarkable man, how great was his dedication to science, and how great was his heroic deed. No one ever heard a complaint from him. Confined to his bed and breathing through an iron lung, he remained the same Tamm whom we all knew. No, he was even kinder. We all have a right to be proud of him."[28]

The ceremony over, the weather changed abruptly: the skies darkened and it began to snow. Big heavy snow-flakes fell onto

[28] Cited from: *Academician Igor Evgenievich Tamm.* Moscow, Znaniye Publishers, 1973 (in Russian).

the ground. It seemed symbolic. I remember someone standing at the open grave saying: "It looks as if nature herself has rebelled against his death."

When I think about people whose loss is never lightened with passing years, I recall Zhukovsky's verse:

> Say not bereaved: they're no more,
> But praise the Fate that they were here.

All those who chanced to live alongside Igor Evgenievich Tamm, to learn from him and meet him should be grateful to fate.

APPENDIX

LETTERS OF I. E. TAMM

In reply to an appeal by this collection's creators to all the contributors to include any letters of I. E. Tamm in their possession, I will report all I know. Igor Evgenievich — after the War in any event — wrote very little. Once, speaking about his own family, he let drop the following remark: "We are all suffering from agraphia." Nonetheless, there is an opportunity to reproduce here several of Tamm's letters. Copies of three of them were kindly sent to me from the Leiden Museum of the History of Science by Mr. A. Y. Engberts (I would like to express my gratitude to him). All three are reproduced in full. Beyond this, some two dozen letters written by Tamm to my mother, S. I. Frenkel, are in safe keeping at the Leningrad branch of the Academy of Sciences' Archives.[29]

Many of these letters Tamm wrote on his own behalf and on behalf of Natalia Vasilievna. Reading through them, the reader will be captured by his remarkable human qualities, by his alacrity to rush to people's aid, to support them in a difficult moment, cheer them up. How well-travelled he was likewise is revealed. He travelled much through the Soviet Union (Kamchatka, Irkutsk, the Kola Peninsula, the mountain ranges of the Caucasus, Kungey Ala-Tau and Lake Issyk-Kul), and ventured abroad. His letters begin in the same way, as though asking for forgiveness: "I couldn't reach Leningrad by phone,"

[29] Archives of the USSR Academy of Sciences, f. 953, item 3, pp. 1—30.

he once wrote. What a happy event! Today's highly perfected means of communication have resulted in the dying out of the letter as a cultural factor. It seems to me the pity of all this is so evident in the excerpts reprinted below in which the living words of Igor Evgenievich ring so clearly.

LETTERS TO P. S. EHRENFEST

Apartment 12,
5 Herzen St.,
6th January, 1930
Moscow

Dear Pavel Sigizmundovich!

I am writing you to remind you of your promise to send me Tanya's[1)] second letter on Einstein's theory. I promise to return it to you by the specified time (should I send it registered mail?).

Mandelstam told me about the debate he had with you on Ungenauigkeits Relation[30] and about your interpretation of Heisenberg's example taking account of des Intensitätsabfalles der Diffraktionringe[31] in the microscope, of which I am very fond. The day after you left Mandelstam thought up a new illustration for der Ungenauigkeits Relation that goes beyond Heisenberg's examples — he views the passing of electrons through the Potentialschwelle[32] from the standpoint of Ungenauigkeits Relation. A very interesting perspective. It would be a shame if, as is his habit, he doesn't publish it. Perhaps you could in some way influence him — your interpretation of Heisenberg's example and Mandelstam's new example could make up a small but very valuable publication.[2)]

We all still feel in good spirits as a result of your visit to Moscow,[3)] for which I would once again say thanks.

Yours, Igor Tamm

24 February 1930

Dear Pavel Sigizmundovich!

You must surely have received my telegram. I am terribly, terribly embarrassed. As you already know, I checked my calculation three times before sending you my note. Then I sat down to write out my work in full for printing — in the course of which I always rework *all* computations from the beginning *without looking* at what I wrote earlier. In doing so I discovered that at the very beginning I everywhere mixed up the sine sign! If everything is done correcty, in the final form there is *no* difference from the Klein-Nishina formula![4)]

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[30] Uncertainty relation (German).
[31] Diffraction ring intensity decay (German).
[32] Potential barrier (German).

This entire story is all the more embarrassing in that I have now managed to do the calculations in an elegant form I am fond of. If they are altered *just slightly*, then it is possible, for instance, to calculate the probability that an electron will spontaneously transfer from a state of positive energy to a state of negative energy. I am working on this problem and will finish my computations in a matter of days.

It is terribly unpleasant for me that for the second time I am approaching you with a request to print my work and for the second time with such blunders (last year I failed to symmetrise the wave equation).[5] Please forgive me for this and many thanks for what you have done for me.

Yours, Igor Tamm

P. S. To be sure, everything stated regarding the predominant role of transitions through states of negative energy remains correct. I. T.[6]

23 February 1931

Dear Pavel Sigizmundovich,

I hope you are back from America feeling much enhanced and rested. I can imagine just how interesting the comparison, in the course of a year, between such antipodes as the USSR and the USA must have been for you.[7]

The other day I was informed, at long last, that my planned trip abroad has been approved by the Ministry of Education and I feel almost certain I will be able to leave around the 15th of April. The idea of the trip first came up last December when you were away from Europe; I wrote to Dirac and Jordan and received formal invitations from them necessary for the trip. I am looking forward to revisiting Leiden with which some of my best memories are associated, and seeing you and yours and all of my old friends there. Would it be convenient for you if I stopped off at Leiden for two or three days on my way to Cambridge some time after the 20th of April? I hope to stay at Cambridge until the end of the university term (15—20 June) and then, should it be convenient for you, return to Leiden for a few days, before pushing on to Rostock in July to look up Jordan. Only I don't quite see how this ambitious programme will fit into my schedule: originally I planned on a five months' trip, but I learn now that it will, in fact, be a three months' trip, and I do not think I will achieve anything longer than that.[8]

As was the case before my last trip in 1928 the whole of my family are laid up — my son has just recovered from scarlet fever, my daughter had a bout with a nasty case of flu complicated by inflammation of the car, and my wife is suffering from a flu-induced pneumonia (luckily, in a light form). Further, as in 1928, I am going to be frightfully busy right until my departure, revising the second edition of my *Theory of Electricity,* and this leaves me no time to follow the current literature.

At the moment the work I have co-authored with Shubin[9], one of my co-workers, is in the press. It seems to me that for the first time it gives a more or less complete exposition of the theory of photo-effect in metals and it was a pleasure to work on it.[10] We are going to verify its conclusions experimentally. I also hope to complete, before my departure, one other work which I have been rather long in writing. It deals with the theory of the difference in the intensities of the Stark-effect component when observed along and counter the field that Stern has observed. This phenomenon seems to be of fundamental interest and is

attributable to 1) the presence of a "tail" of the wave function of electrons drawn out by the field outside the atom; 2) non-stationarity of the state of the atom in the electric field (spontaneous ionization). However, the quantitative calculations involved are complicated and have not been completed yet.

So, I am looking forward to seeing you in April. Kind regards to Tatyana Alekseyevna,[11] and Tatyana Pavlovna[12] (I hope to reply to her letter verbally) and to all my friends (Ruthgers! I would like to have his address)[13].

Yours sincerely, Igor Tamm

P. S. Sorry for the handwriting, I do hope you can read my scrawl ...

NOTES TO THE LETTERS TO P.S. EHRENFEST

1) The daughter of P. S. Ehrenfest, Tatyana Pavlovna van Aardene- Ehrenfest, a Dutch mathematician. She studied Einstein's work in the late 1920s at Göttingen under the tutorship of Born, who thought highly of her mathematical abilities.

2) No publication came off. This remark is significant as corroborative evidence showing how fast L. Mandelstam could unravel paradoxes related to electron corpuscular-wave properties, so uncommon in the first years of quantum mechanics. Subsequently Tamm himself said much, both verbally and in print, about that in connection with the Einstein-Bohr polemic.

3) P. S. Ehrenfest came to the Soviet Union in December of 1929 and spent over a month in Leningrad; and it was from Moscow that he left for Holland. During that sojourn he also managed to visit Kharkov, where the Ukrainian Physical-Technical Institute had just started functioning.

4) Tamm's work "On the Interaction of Free Electrons with Radiation According to the Dirac Electron Theory and Quantum Electrodynamics", published in the German journal *Zeitschrift für Physik,* 1930, 62, p. 544 (*Tamm I. E.* Collected Works. Vol. 2, p. 24). Using an exact quantum-mechanical formalism, Tamm obtained the Klein-Nishina formula known previously but not based on rigorous procedures (within the framework of the Bohr principle of correspondence).

5) Tamm's work "On Rotating Electron Electrodynamics" published in "*Zeitschrift für Physik*", 1929, 55, p. 199 (*Tamm I. E.* Collected Works, Vol. 2, p. 5).

6) The circumstance which Tamm indicates in the postscriptum to his letter was of special significance since in 1930, prior to the discovery of the positron (1932), the state of a negative energy electron (Dirac's "hole") was believed to be an arbitrary construction. It was Tamm who, for the case of light scattering off a free electron, proved it indispensable to take into account negative energy states.

7) Upon his return from the Soviet Union, Ehrenfest stayed for several months (from June 1930 to February 1931) in the United States.

8) Tamm's trip abroad did take place.

9) Semion Petrovitch Shubin (1908—1938), I. E. Tamm's pupil, Dr. (Phys. & Math.) and Professor. See the article by S. V. Vonsovsky, M. A. Leontovich and I. E. Tamm (Uspekhi Fisicheskikh Nauk, 1958, 65, p. 734).

10) The work "On the Theory of Photoeffect in Metals" (*Zeitschrift für Physik*, 1931, 68, p. 97, translated into Russian in: *Tamm I. E.* Collected Works, Vol. 1, p. 196).

11) Tatyana Alexeyevna Afanasieva-Ehrenfest (1876—1964), a Russian mathematician and P. S. Ehrenfest's wife; authored a number of works on thermodynamics and statistical mechanics.

12) See Note 1).

13) K. Ruthgers, a noted Dutch physicist and pupil of P. S. Ehrenfest.

EXCERPTS FROM LETTERS TO S. I. FRENKEL

April 24, 1953

My workload is enormous, there was bad fatigue in February, but Uzkoye[33] quickly put me back on my feet.

October 17, 1953

... Natasha and I are at Tskhaltubo (a spa in Georgia — *Ed.*) taking treatments for all kinds of ailments, and so we have decided to write you a letter (for it is not often that we are together in conditions when it is possible to write!), send our greetings and enquire about your health.

... The only thing we have enjoyed here were two thrilling trips back to the Georgian antiquity of the 11th and 12th Centuries A. D. We were particularly impressed by the magnificent ruins of a temple built in 1003 on a high hill affording a wonderful panorama of Kutaisi and its environs.

In between Moscow and Tskhaltubo, I made a short but pleasant trip in the mountains and took dips in the sea at Gagra for two weeks.

December 31, 1955

I have been running a temperature of over 38°C for four days, feeling very bad; today the temperature dropped, but the doctors found a little focus,,,, and they are going to put me on penicillin. These are trifles, of course, but I am not used to being sick.

June 23, 1958

... I am flying off to Geneva tomorrow. My head is turning — have been studying ways of detecting atomic explosions so as to act as an expert; also getting ready for the elementary particle conference to be held concurrently with the expert conference, and I am in the thick of other things. To cap it all, I have served as an expert on the Academy's election commission etc. My head is reeling, I am up and about from 8 in the morning to 1 after midnight.

November 19, 1958 (on a postal order)

... Don't get upset about this remittance, I hope you'll get the royalties in a day or two; I'll be sending more if need be.

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[33] A spa resort of the USSR Academy of Sciences near Moscow.

January. 23, 1959. Uzkoye

... It is really bad that I am not with you[34]. I would have come yesterday without fail had I not been immobilized temporarily. I had no flu, we made it up not to worry you... you see, on Sunday, the 18th, I had a bad fall while skiing, broke a jaw bone (or rather the jaw appendix, the jaw bone has developed a crack). I am quite well now, but the doctors (we are at Uzkoye after all!) prefer to play safe and they have put me on a sick regimen: to keep in bed, penicillin, a bandage on the head to prevent me from opening the mouth too wide so as not to dislocate the broken bones etc. I'll break loose today for a walk... but... in the first half of February I will definitely come to you.

June 16, 1959. Khosta

... The new giant rest resort of the Art Foundation in the city park; on one side it is flanked by an open-air theatre, and on the other by a highway going uphill. There is a terrific noise from the traffic. This "acoustic treatment" drives me up the wall occasionally. I am working and taking dips in the sea, but with no results so far — either there is no fish, or there is no gun.

August 2, 1959

... In Kiev (at an International Conference on the Physics of High Energies and Elementary Particle,— *V.F.*) they told me that one major American experimental physicist, who has studied the μ-meson magnetic effect, boasts he has found Yasha's work (of Y.I. Frenkel,— *V.F.*) dating back to the 20s and dealing with a rotating electron, it proved so helpful for navigating in the theory of the phenomena he studied.

May 20, 1960

I've got so much on my plate, am very tired and feel small, because my work which has taken so much effort and energy in the last six months has yielded zero. I feel I could do with a rest but cannot manage it as yet.

V. L. Ginzburg

ABOUT IGOR TAMM

Those who cherish the memory of Igor Tamm have not neglected their duty: several obituaries, notes and a memorial volume of works[1] have been published and dedicated to I. E. Tamm, as has

[34] On January 23, 1952, my father passed on; from 1953 on Tamm came on that day to Leningrad almost every year.

[1] *Problemy teoreticheskoi fiziki: Sbornik pamiati I. E. Tamma.* Moscow, Nauka Publishers, 1972.

a bibliographical index of his works[2] and, most important of all, a two-volume collection of Tamm's works.[3] His textbook *Fundamentals of the Theory of Electricity*[4] has also been republished. Yet I, for one, had no notion whatsoever of writing recollections or notes dedicated to Tamm before a telephone call from the editorial office of *Priroda* (Nature) magazine.[5] It came out of the blue and, perhaps, for that reason was so effective: why hadn't I even thought of writing my recollections of Tamm when others had found it a natural thing to do?

No sooner was the receiver down than these words came to my mind: "... never send to know for whom the bell tolls; it tolls for thee." I did not remember any more lines from the poem (by John Donne, a contemporary of Shakespeare) which Hemingway borrowed for the title of one of his novels. Having read the whole of the epigraph one may conclude that Donne had in mind something quite different from memoirs. Still, I remain of the opinion that it is primarily the toll of the bell for the authors and not only for those outstanding people whom they are remembering that is so typical of contemporary recollections. If no toll can be heard, then either the author was not the right man for the job or he tried hard to remain in the background, did not want to write about himself. In itself the latter intention is quite natural and praiseworthy, and yet, unfortunate as it may be, in practice it is often artificial and is liable to come back on you: it empties the recollections of sincerity and genuineness.

Hence, writing down your recollections implies writing about your own self and involves the risk of making you appear vain or egocentric. The task is made even more difficult when people who are not natural writers have to take up the pen.

For all that, I decided to write about Igor Tamm to remember a few episodes and to try, at least to some extent, to explain why his image is so particularly dear to me. It goes without saying that this attempt is nothing more than an array of simple unconnected notes and observations; nor will there be much about Tamm as a physicist even though physics was central to his life, for it is hardly possible to give a complete picture of a phy-

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[2] *Igor Evgenievich Tamm (1895—1971)*. 2nd ed. M., 1974 ("Materialy k bibliografii uchionykh SSSR. Ser. fis."; Vyp. 16).

[3] Tamm I. E. *Collected Works*. Moscow, Nauka Publishers, 1975.

[4] Tamm I. E. *Osnovy teorii electrichestva*. 9th ed. M., 1976.

[5] This article was published in *Priroda* (Nature monthly, 1975, No. 3, p. 65). Only insignificant changes have been introduced into the present text and one episode has been added.

sicist outside and without physics (the same goes for other occupations). By way of justification might I just mention that Tamm's contribution to physics and his personal qualities as a physicist have been adequately treated in the above-mentioned publications.[6]

So on to recollections. Let me begin with a time when I saw Tamm only from afar. When such is the case one's memory tends to retain not the central events but secondary, even insignificant detail.

* * *

From 1934 to 1938 when I was studying at the Faculty of Physics at Moscow University, Tamm headed the Chair of Theoretical Physics and all of us, the older undergraduates in particular, knew him. He fairly rattled along during his lectures and occasionally got into a muddle (he had probably not specifically prepared the lecture). Still, what he said was much more informative than many other lectures, smooth and rounded though they might have been. He was vivacious, ingenuous in his explanations and extremely profound. A good example of the style of his lectures can be drawn from his *Fundamentals of the Theory of Electricity* course, the textbook has long been a favourite for many physicists (certainly for me).

L. I. Mandelstam was teaching at the Faculty of Physics during the same period. His was not a standard course but something much broader; the lectures were optional but the auditorium was usually packed, teachers and instructors (Tamm included) being regular attenders. A number of Mandelstam's lectures have been published. One of his courses dealt with paradoxes. The students were split into groups, each led by one close to Mandelstam. Tamm was the mentor of our team and we were to analyse some paradox concerning electrodynamic forces (to my regret, I don't remember the paradox itself).

We got together in Tamm's study (which happened to be one of the few half-empty rooms belonging to the Theoretical Physics Chair) and he began formulating the paradox. But hardly had he finished the description when one of us shouted the solution. As I said before, I do not remember the paradox itself. What I do remember is that Tamm was quite upset and even suspected us of having known the answer beforehand.

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[6] See also the short biographical essay at the beginning of this book — *Ed.*

Moreover, he seemed to disbelieve us when we assured him that we really had not known.

This kind of thing would happen from time to time. Truly generous and often all too trusting, Tamm sometimes for no apparent reason became suspicious over some trifles. For instance, somebody once said "a prieri" instead of "a priori". This stayed with me and I began to use it as a joke. But I stopped saying it for good after Tamm corrected me ("a priori" not "a prieri", Vitaly) with an expression of his face that made it quite obvious that he did not believe me when I said it was a joke. Why does my memory hold such trifles? It is difficult to say — probably because they seemed so unexpected. And it is only because I remember them with exceptionally warm feelings, and not in a spirit of "even the Sun has its dark spots", that I write about them now.

Tamm was a mountain climber, but my experience with him in the mountains was in the period when his climbing was minimal. In 1945 we were in the Pamirs, near Murgab, at the Lebedev Institute's Cosmic Ray Laboratory Station at an altitude of about 12,350 feet. Tamm, who was just getting over an illness, kept taking medicine and was not at his best. Long walks were difficult for him, and he suffered from this. Besides, he was already 50, not the best age even for easy climbing. A proud man, never willing to give up, he was upset when I, a complete beginner in the mountains, was faster than he. Incidentally, when he had completely recovered he went back to climbing and kept it up till his 70th birthday in 1965.

I mentioned this episode for the following reason. There is a considerable disparity in attitudes to younger and older people. I remember seeing Tamm in 1935 or 1936. He was outside the Institute of Physics at Moscow University telling a group of his acquaintances how he had fallen while skiing in the mountains and got a large bruise that was so bad that almost a glassful of blood had had to be pumped out of it. At that time it seemed to me that Tamm was an elderly man who ought to stop such heavy exercise. And he was only about 40 then. It is a common attitude: anyone 20 or more years older than oneself seems elderly and this creates a feeling of distance. But not the other way round: such feelings are rare towards one's students or postgraduate students who are some 20 years one's junior, particularly where science is concerned. This was very much so with Tamm; he felt absolutely at ease with people much younger than himself and he felt them, though not all of them of course, his equals.

In this country the 1920s and 1930s saw heated debates on methodological problems connected with the theory of relativity and quantum mechanics on the one hand and with the fundaments of classical physics on the other. The "mechanists" denounced as idealists for example those who did not believe in the existence of mechanic ether. I will not go into those debates which were covered in the press but will mention just one theoretical discussion at the Physics Faculty in 1936. The Great Physics Auditorium was packed to capacity and the main argument was whether electromagnetic energy propagation was possible without "mechanical transference" of something in space. I. E. Tamm, G. S. Landsberg, B. M. Gessen and I do not remember who else spoke in favour of the electromagnetic theory without mechanical transferences and their many opponents pelted them with a whole variety of accusations, demagogical claims and even indecent comments. One such argument drew indignant protests from Tamm who never allowed himself anything of this sort. Tamm never used bad language and was never known to tell a dirty joke. This was not showing off or being strait-laced, it was just the way he was.

Tamm's part in fighting vulgarisers and pseudo-scientists is very significant. This needs pointing out even though this is perhaps not the place for going into detail. As to physics I would refer the reader to Tamm's article in the *Pod znamenem marksizma* (Under the Banner of Marxism) magazine.[7] In later decades, when physics no longer needed defending, Tamm stood up for the long-suffering genetics and molecular biology with the same enthusiasm and determination.

* * *

I will now tell how from a much respected professor, Tamm became a close and very dear friend of mine. This is a very difficult subject to write about, especially since in this context I shall have to say more about myself than about Tamm. That is, of course, a superficial problem for my primary intention is to illustrate how valuable his considerateness and helpfulness were.

In the 3rd or 4th year in a Physics Faculty students usually have to specialise and for many this is a hard and often frustra-

[7] *Pod znamenem marksizma*, 1933, No. 2.

ting period. Not for everybody, of course. Some are quite certain of what they want and, moreover, are confident of success. But others either are unsure or totally unaware of their abilities. Theoreticians tend to have more problems than experimental physicists. If a student chooses theory his work may "get stuck" and he finds himself back to square one. Experimental physics seems to be a safe field: if one fails, one can at least hope to get a job as a laboratory technician.

I was an average good student, though with obvious lack of mathematical abilities. So, much as I wanted to, I could not make up my mind to become a theoretician — it was commonly assumed, and not without reason, that a theoretician had at the very least to be on good terms with mathematics. So I chose optics instead and became engaged in rather sophisticated experimental research — measuring the angular distribution of canal ray intensity. The resultant diploma work was not much to boast about but the general opinion was that I should go on with the research; in any case Landsberg, head of the Optics Laboratory, recommended me for the postgraduate course. The circumstances, however, were such that the new postgraduate physics students were drafted and deferment was some time in coming. So experimenting had to be interrupted and I started to theorise. In the process an idea occurred to me concerning the mechanism of radiation under the collision of an excited atom with a charged particle.

I took my idea to Tamm, if I remember rightly on September 13, 1938, after a lecture. Let me warn you here in advance that my argumentation rested on an error, but this kind of problems was then almost unknown and Tamm missed it. Luckily for me, he treated my idea with interest and enthusiasm. Moreover, he inspired me with his enthusiasm too, advised me on reading matter and calculations and asked me to keep him in touch on the results.

The word "asked" does not seem quite fit. There wasn't any "asking", of course; it simply became clear that Tamm was really interested and there was no need to feel shy about coming to discuss things with him. In general, he was helpful and open. His attitude towards a student whom he hardly knew was as to a colleague.

I felt inspired; I literally started a new life. It turned out that I had come onto a range of problems in radiation theory which were still little known about and apparently did not need sophisticated calculations for clarification but rather more accurate definition followed by integration of just a few simple equations for harmonic oscillators. This may be viewed as another proof

that theoretical physics is not dominated by mathematics and that the cart (the calculations and the formulae) is still drawn by the horse (physical images and ideas), not vice versa. This statement should of course not be taken as an attempt to deny the great importance of mathematics in theoretical physics which would be absurd. My sole intention here is to emphasise (since it sometimes is disputed) that eventually it is not mathematics that calls the tune in physics. Tamm thought exactly the same.

So with his backing I became a theoretician. I would also like to give Landsberg his due. Originally "his" student, I "betrayed" experimental optics in the very first month of the course by taking up the postgraduate course under Tamm. But Landsberg afforded me complete freedom, the most important thing for him being that I worked efficiently. I received my Master's degree in 1940 and took up a doctoral course at the Lebedev Institute; this time Igor Tamm was officially my science supervisor.

But such titles really played no part at all. Everybody — experienced scientists, doctoral candidates and postgraduates at the Lebedev Institute's Theoretical Physics Department set up by Tamm in 1934 (the year the USSR Academy moved to Moscow), was an equal partner. Only one thing mattered — what you did and how good you were at it. Tamm worked with some postgraduate students on the same research. As for me, I was not lucky enough to work on a joint research project with him on either the Masters or Ph.D. courses (the latter ended in 1942). This, however, did not mean that he took less of an interest or paid less attention to me: we kept discussing scientific problems and my research results.

It was somewhat later (but still during the war years) that we came to be co-authors. Tamm was then busy with a theory of electromagnetic properties of laminated cores composed of intermittent metal-dielectric layers (according to N. D. Papalexi, such cores could be used in aerials). I think I found some inaccuracy in his preliminary calculations and then somewhat improved on his work. Anyway we published an article jointly. In the other case, unlike the first, it was I who attracted his interest to the relativistic theory of particles capable of having various spins, a problem which took us several years of hard work.

Working with Tamm was sheer pleasure. There was never any problem of ambition, division of labour or the like. Only one thing was difficult to do with him — co-authorship in writing articles. He disliked writing and he kept inventing pretexts

for putting off the final full-stop (he himself admitted to "agraphia"). It goes without saying that both our articles were prepared for publication by me but even that didn't help much. It took us almost two years to complete the article on spins and it was only in 1947 that it appeared in the *Journal of Experimental and Theoretical Physics*.

* * *

A few words now about Tamm as a physicist, though strictly speaking it is off my subject.

Tamm was a theoretical physicist with broad horizons: he was well up in every key area of physics and worked in many of them. But at any given moment Tamm usually was fully absorbed in just one thing; he tended to get carried away and spend all his time and energy on solving whatever problem had most recently captivated his imagination. His industry was remarkable: it was not unusual for him to work through the night or holidays. Work gave him pleasure when it aroused his imagination. To do this a problem had to be real and of fundamental importance. Such problems are particularly difficult to deal with, for one can work on them for years without achieving any significant result. Yet this did not seem to worry him; he was never guided by such considerations as being able to write an article or by a need to "produce more end-results" in general.

* * *

Often scientists have not only ambition but vanity. As far as I could see, vanity was totally alien to Tamm. He never sought awards or honours. He was pleased at winning a Nobel Prize, but nothing more. Incidentally, Tamm did not consider the Vavilov-Cherenkov effect theory, for which he and I. M. Frank were awarded the Nobel Prize, to be his best work (in his opinion it was the theory of nuclear beta-forces).

As to ambition, this word is actually not sufficiently monosemantic. Too often we attribute it beyond people who seek social standing and an opportunity to govern others, to those who wish to do good work and see it recognised, thus, so to speak, asserting their own personality. In this latter sense, let us call it "good ambition"; usually it is even necessary and represents a condition for success in one's work. What a host of talented men have not realised themselves owing to laziness, indifference and, in fact, a lack of good ambition!

To my mind, Tamm possessed this good ambition, as well as self-esteem and pride. His was an ambition that never hurt others. I do not know how to put it more exactly. For example, if he made a blunder when playing tennis or some other game there would follow, almost immediately, a gesture of displeasure. Similar reactions were obvious when he was losing a game of chess. I have already mentioned his dissatisfaction if he felt his mountain "walks" were not going well enough. But that revealed something of a child in this grown-up and even elderly man whose pride kept him from complaining about an illness or a pain and made him hold out.

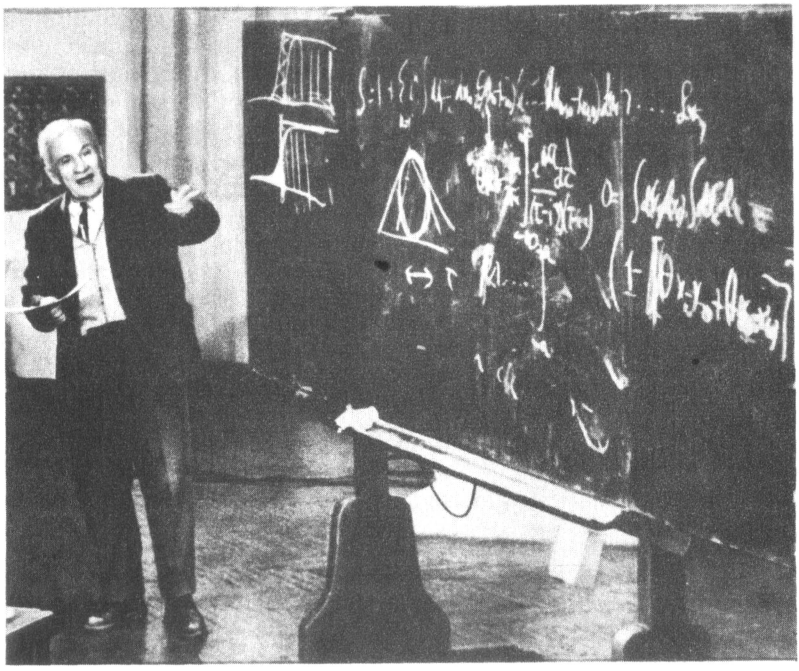
The last three years of Tamm's life can only be called tragic. He developed lateral amiotrophic sclerosis in 1967 and from February 1968 paralysis of the diaphragm riveted him to a respirator, or, to be more exact, to machines which he could change to be able to sit at the desk and work with the aid of a portable device made by an ingenious craftsman. It was with a smile marred with bitterness that he used to speak of himself: "I'm like a beetle on a pin." Yet the first two years he did a lot of work, played chess and was happy to have visitors. Moreover, he became gentler: the illness neither crushed nor embittered him. Earlier he had been more withdrawn, probably thinking it improper to show his feelings too much; now that he was seriously ill they were manifest more freely.

From 1948 to 1953 Tamm spent much time working far away from Moscow, often alone, without his family. I meantime for the most part lived in Moscow and my wife in Gorki. This was a period of much work and little pleasure which probably explains his remark during one of our meetings in Moscow (it seems to me in 1950). He said something like this: "Vitaly Lazarevich, I happened to be reading Omar Khayyam and thought of both you and myself."

> The days decline
> Without love and wine.
> But in the Book of Destiny of mine
> They are struck out just the same.

I remembered this text quite well but failed to find anything of the kind among Khayyam's quatrains.[8] However, this is not what matters: it gives me pleasure to state that in the years that followed Tamm had many full days to live.

[8] According to L. I. Vernsky (Tamm's grandson), there are several hand-written verses "from Khayyam" (no sources cited) among Tamm's personal papers, one of them even appeared in three variants.



At a seminar of the Theoretical Physics Department (1965)

But let me return to certain features which are so typical of "creative intellectuals". There are people who literally fall ill if they are not mentioned or quoted, to say nothing of cases when something is borrowed from their work without "due reference". I was never able to notice anything of the kind, not even a hint of it, in Tamm for he was well above any considerations of trifle priority.

A good example of that is the elections to the USSR Academy of Sciences. In 1946 there was every reason to suppose that Igor Tamm would be voted a member. Everyone considered him the No. 1 candidate, and he had well deserved it for a long time. Yet he was not elected owing to circumstances which had nothing to do with science. I have seen quite a few "non-elected" (for some or other reason). They were people who carried around their "wounded pride". Some fell ill, others quarrelled with those who had "offended" them and sometimes even with people who had nothing to do with the elections. Everyone must know

what hurt self-esteem is. But as for Tamm, he showed no sign whatsoever that he felt hurt. I think that, though indeed upset and offended, he did not suffer very much from this obvious insult (which was exactly what it was). Tamm's sense of humour also helped and the fact that he knew the true value of things (though that does not always help people when something concerns them personally). I remember Tamm once telling me how the had congratulated one physicist just elected to the Academy: "You know, he thanked me as solemnly as if it were a matter of vital importance; just think, that man totally lacks any sense of humour!"

Tamm's self-esteem and pride were always in the best sense and never petty. Meanwhile his feeling for principle, combined with sense of duty, with what is called "noblesse oblige", compelled him to fight ignoramuses, to write certain protests etc. I am using the word "compelled" because I am sure he did not like writing "abusive" articles or taking the floor on such matters. But if he thought that he ought to speak up, he did not vacillate.

* * *

That in 1934 Igor Tamm set up the Theoretical Physics Department, later named after him, at the Lebedev Institute has already been mentioned. The initial group of 5-6 has grown into one of the largest scientific units in the world (at present there are about 60 people in it, with a much larger number of its former postgraduates and researchers working in other places). And during the 50 years of its existence there has not been a single case of a serious human conflict in it, not to mention a scandal. Few colleges can claim this and the credit must go to Tamm. Such practices as the supervisor's (or anybody else's) name being added to another person's work were unheard of there as were red tape, compulsion and disrespect for juniors. On the contrary, friendly support and criticism (although sometimes sharp and passionate) flourished; so did concern and freedom to think and work. That was the whole simple "secret" to Tamm's education. Not to stand in people's way, to treat them well, to encourage the young, to give them freedom and timely advice — these frequently comprise all that is needed for a good friendly atmosphere at work. It is only to be regretted that such seemingly simple requirements are far from always applied.

The nearer to the end I get, the more obvious the inadequacy and incompleteness of my recollections. On reading the manuscript, a friend of mine, for instance, has asked: "How is it

possible to write about Tamm without mentioning his charm?" I cannot disagree with him but I can only answer his question with another: "How can one write about his charm? Or how can one write about so many other qualities in a person?" It often happens that even seemingly insignificant details and episodes are illustrative of something greater and so help the reader understand something really important. I can only hope on this.

Igor Tamm was a very good theoretical physicist and produced first-rate works. He wrote an excellent textbook, educated many good scientists, and fought for a truly progressive and modern face to science. All this is true, indeed, and very important. But that is not all. Were what has been said the whole story, our great respect could easily be justified; yet a person is loved most of all for something else, for his human qualities. Speaking about Igor Tamm, I would define the attitude towards him of both myself and many, many others as an alloy of respect and love.

E. A. Kazakova

CLIMBING MOUNTAINS TOGETHER

We met in 1955 and quickly became friends. We often went mountaineering in the same group. He was a goodhearted and agile man, who was a fascinating narrator and an ideal listener with a sharp mind stretching far beyond physics and mathematics. He was sincere and simple in his relations with people, and never forgot the "filter" of tact. Igor Tamm was a true colleague in the mountains and a super companion. Chess was not his only pastime. He liked an amusing idea, an unsolved riddle, unexpected events, adventure — everything that fed his fantasy and imagination. For him a trip that had this was the best possible rest. His holidays usually lasted five or ten days and sometimes, although rarely, twenty. But after five or six days he would often become restless, and leave the camp with notebook and pen. The "bombshell" normally came after ten to fifteen days: then Tamm with a guilty look would talk about something urgent in Moscow and would invent most sophisticated transport combinations to get out of the mountains. To go with him to distant places could be quite a risk.

* * *

"So here we are acquainted at last and a pity it's taken so long. About 20 years ago I remember I was going to join you in a climb but something went wrong. Well, this time nothing will stop me going to the Pamirs."

This is how Tamm and Boris Deryagin joined our trio of Sergei Lukomsky, Georgi Vedenikov and myself. We were planning to pass from the Vanch River valley via Academy of Sciences Ridge to the upper reaches of the Fedchenko Glacier. This region, first visited by the Pamirs Expedition of 1928, was left in peace for many years for being remote. We resolved to use the Abdukagor-II Pass; I. G. Dorofeev, a topographer, had discovered it from the Fedchenko Glacier but had not crossed it to the western side. Our small expedition was sponsored by the Geographical Society, a popular and respected organisation but with zero resources.

To go so far into the mountains at that time was not an easy undertaking. The first attempt at a road had been cut through the rocks on the Soviet side of the Pjandzh River and in some places, under the overhanging ledges of rocks, our 1.5-ton lorry brushed the walls. To the right there was a real mountain chasm, with a noisy river at its bottom. We sat in the lorry chatting but we kept our wits about us and pulled our heads in every time the shadow of an overhanging rock swam over the lorry. Tamm often had to take off his Balkar hat because its brims hid the dangers of the road. He replaced it with a handkerchief with knotted corners.

Only on the fifth day after departure from Dushanbe did we reach our base camp on the right bank of the Abdukagor River, 11,400 feet up, not far from the tip of the Medvezhi Glacier which covered the river valley that year. From here we would have to go on foot with pack-horses. In the morning, while we were repacking our load, Tamm took all our climbing boots, unlaced and greased them. Then he volunteered for kitchen duty. We were to return the hospitability of the geologists who had treated us to fried wild ox the night before. Both Tamm and Deryagin seemed to be new to campfire cooking. Both of them took the job very seriously. I advised them on recipes and methods, no more. The fact is that each of the five of us had highly individual ideas on cooking.

First, Deryagin[1] carefully inspected our supplies, made a list

[1] Physical Chemist, Corresponding Member of the USSR Academy of Sciences — *Ed.*

of possible dishes, asked me for exact quantities, temperature and cooking times, and so on. Soon he had turned the art of camp cooking into a branch of physical chemistry.

In his desire to catch the art of camp cooking, Tamm fantasised, ventured, went wrong and got upset, raked back and forth between fire and our store under the rock in search for new dressings, remembering things he had eaten in the European restaurants and trying to make tinned pork and beans into steaks and lobsters. The upcome of it was that Tamm's dinner turned out quite a success. Elegantly served on leaf-covered stones, the meal was substantial, tasty and attractive. Tamm presided satisfied and proud of his success in a new field.

The next day saw us in the upper reaches among glaciers and snow-capped peaks. Tamm was much older than the rest of us and had done relatively little climbing in the last few years, and our route passed some 13,000 feet above sea-level. But even though it was more difficult for him than for the rest, even though he sometimes fell a little behind or had to sit down for a rest, he strictly observed the rules of both the group and the caravan. Two days later we reached the geologists' upper camp at 14,000 feet, a spot surrounded on all sides with ice-falls and steep slopes of rock and snow which produced no shortage of stone-falls and avalanches. The weather got worse, at times heavy snow covered our tents and the gusts of cold wind chilled us to the marrow.

As soon as the weather changed the camp came to life. We re-inspected the loads, prepared our rucksacks for the climb to the Fedchenko Glacier and explored the path to the pass. Tamm had discovered there were some good young Tadzhik chess players working with the geologists and hastened to organise a chess tournament. Three times a day he measured and recorded the air temperature which was his duty in the expedition.

After three days the weather settled down at last and it was time for us to say good-bye to Tamm. He had been informed at Vanch village that he had to return to work. We pulled on our rucksacks and started up towards the Abdukagor Pass. For a long time we could see a lonely figure standing at the edge of the moraine waving a white hat.

* * *

The expedition most in the spirit of Igor Tamm was probably the one he organised to the Mata-Tash cave in the Eastern Pamirs, close to the border with Sinkiang.

In my "Pamirs-1957" scrapbook I have a thick bunch of letters,

replies to articles published in the *Ogonyok* magazine. The letters contain drawings, sketches, descriptions and advice on how to conquer the inaccessible cave. The writers were soldiers, athletes, workers, pensioners, state security officers, students at the Cadet School in Tiuringia, a group of Bulgarians, the staff of the Kuskovo militia station...

In the winter of 1957 I had a telephone call from Tamm:

"My dear Elena Alexeyevna, don't you think it's time to solve the mystery of the Mata-Tash treasure?"

Next day, sunk deep in leather armchairs in his study, we had a conversation that prompted the expedition to Salyk-Tash Ridge in the Rangkul region of the Pamirs. The entrance to the Mata-Tash[2] cave is in the middle of a rock wall on the northern slope.

A legend of the Kirghiz Pamirs about the Rangkul cave[3] was published in *Turkestanskiye vedomosti* and *Izvestia Rossyiskogo Imperatorskogo Geograficheskogo obshchestva.*[4] About 200 to 300 years ago a large troop of warriors came to stay in the Rangkul Basin for the winter. But soon deep snow covered the ground, the horses had no grass to feed on, and food supplies began to run out. The shrewdest of them fled on the best steeds, far-sightedly cutting the tendons of the rest. As death was inevitable, the remaining warriors decided at least to save the looted treasure and chose an inaccessible cave high in the rock. They hacked their horses into pieces and froze them up to the wall of the rock to make a "staircase" by which to reach the cave. To protect themselves against the freezing cold they moved into another cave on the southern slope of the ridge where they soon died of hunger. The spring melted the "staircase" and the cave again became impregnable, faithfully keeping the secret of the treasure entrusted to it. Many a time the local Kirghiz tried to get into the cave but, frightened by a "shaitan who stuck his head out of the cave", they fell down and got killed.

In 1949, A. V. Bleshunov, a climber, made a solo attempt on the Mata-Tash cave using safety hooks. He fell some 20 or 30 yards below the cave; though not badly hurt, it put an end to his attempt. Two years later another attempt was made —

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[2] *Matá* (Pers; *mató* — tadzh.) — the name of hand-made cloth, snatches of which were found at the base of the Tash-Kamen mountain. They were most likely blown out of eagles' nests by the wind.

[3] Abridged version.

[4] *Turkestanskiye vedomosti*, 1898, No. 92; *Izvestia Turkestanskogo Otdela Imperatorskogo Russkogo Geograficheskogo obshchestva*, 1915, Vol. 11, issue 2, II.

this time by climbers from the Sport Club of the Turkestan Military District — and again in vain: the cave could not be reached from above or from below.

* * *

The first meeting of the treasure hunters at Tamm's was a stormy affair.

The sceptics argued:

"There is no treasure in the cave, so there is no need to try and get in there."

We the enthusiasts argued that it would be interesting to prove or to disprove the legend, and that it was a challenge.

The doubters got rid of, eight remained. We vowed to laugh if, after all our pains, we only found down guano in the cave.

Our expedition required substantial resources. Only three members of the expedition, Fyodorov among them, were of "sound standing", the rest being young and impecunious. Tamm put up the lion's share of the expenses from his recent State Prize. Many other cares were also taken on by our chief. All sorts of friends and contacts were urged into action, and soon we had 400 yards of flexible wire rope, 500 yards of nylon rope, 1,000 yards of telephone cable, a winch, parachute slings, a block-brake, clamps, rope-ladders, safety-hooks and many other essential things.

Boris Shlyaptsev set off earlier for a mountain camp in the Caucasus; from there he would go on to the Pamirs. On July 10, five climbers took a train to Osh, their two compartments packed with strange-looking equipment. Tamm and I flew from Moscow to Tashkent in one of the first TU-104s which deeply impressed us with its luxurious silk wall upholstery, Karelian birch tables, delicious *hors d'oeuvre* and fine wines. We were in a holiday mood. The night was receding in the west and the morning sky was lighting up ahead of us. Tamm busied himself with chess. Four hours later the plane landed in Tashkent.

The Asian sun in Leninabad was truly roasting as sleepy and hungry we hurried to the plane-tree and the three empty airport canteen tables under it. After half an hour of patient waiting (fortunately the plane was in no hurry either) a plate of bread was set on our table and by tacit agreement we took a slice each, which made us hungrier still. And still nobody from the kitchen. When a young local, a regular, came over and sat down at our table, the curtain opened immediately, as if by a wave of the magic wand, and a plateful of steaming pilaff swam in the air and landed before him. He smiled with satisfaction, took the spoon and ... stretched out his hand towards the plate

with bread. But Tamm smashed his fist down on the table and shouted: "Don't dare take our bread!".

Our food arrived with the "Boarding" announcement. We ate in silence, ran for the plane in silence and reached Osh in silence.

When we got off the plane Tamm said: "I did something awful. Please, forget about it — if you can." I nodded agreement but even now, twenty years later, this loud cry of pain and indignation against injustice still rings in my ears. Tamm had a quick temper but he was also an example of politeness and good manners. I often saw him angry, offended, indignant but never again lose his self-control.

* * *

We were back on the Pamirs, like two years ago, but this time on the east side. We sat in the lorry skidding at every bend in the road now to the right, now to the left. A day and a half later we had left behind us the Alai Ridge, the Alai Valley and were up in 16,500 feet high passes in the clouds. We were welcomed by the physicists from the Lebedev Institute mountain cosmic ray station at Chechekty. The community there was fairly large. The weather was fine and researchers and students alike were in a hurry to get on with their work using devices hidden away in tiny, odd-looking buildings. Tamm's arrival generated a wave of pleasure and enthusiasm: the researchers were keen to talk to him and hear his advice, and students wheedled him into giving a talk on genetics.

The neighbouring biology station housed a group of Leningrad mountaineers looking for the snow man.

Now we were moving east through a wide valley. To the right lay a low ridge with oddly weathered rocks. Tamm was reminiscing:

"Our scientists had an embarrassing incident among those rocks during the first years of the cosmic ray station. One moonlit night they drove to the valley and noticed some mysterious luminous place over there. They stopped the car, climbed up and found quantities of eagle guano in a cleft. To be on the safe side they took a sample and back at the station they put it under the Geiger counter and just imagine: it started clicking hard. That fairly had them going. Everything was dropped. They climbed around all of the ridge and took guano from every nook and cranny. This time, however, the counter remained silent. So they had found nothing. The physicists felt frustrated and con-

fused as I learned from their wall-newspaper when I was in Chechekty last time."

"Did they find out why the rock was luminous?"

"They did. The winds had drilled a hole in the rock and the moonlight, passing through it, created the impression of a luminous halo."

We drive on along the Rangkul Lake. "Look, there is the Mata-Tash Cave," says the driver pointing to a rock of dolomitised limestone. It was about 1,000 feet high. The vertical wall faced North, towards the lake. The entrance to the cave could be seen in the middle of the rock; there was a heap of stones under it looking like some artificial stonework. After passing the lake, our vehicle turned into a wide ravine with a flat pebble-covered bottom. We stopped. An unpleasant surprise was in store: a cloud of insects had followed us from the marshy bank of the Rangkul Lake. We made camp and hung the following slogan borrowed from N. Kastere's book[5] on the canteen tent: "You need not hope to try, nor succeed to carry on."

The ensuing events are described in short passages from my diary. I think it appropriate to quote them here because they may help reproduce the atmosphere of our expedition.

"July 19. It is the early hours; our drinking water is frozen over. The valley is in shadow and everything is gripped in cold. I haven't the courage to get out of the sleeping-bag. Tamm left the tent long ago, took his notebook and pen, and sat down to work in a band of sunlight across the valley. The whistle of the marmots doesn't seem to bother him. He will return to the camp with the sun and that will be the signal for the rest of us to get up.

"Water is at a premium and we have a new system for getting washed: three or four pairs of hands are placed on top of each other and a thin stream of water from a toy garden watering-pot flows down on them from above; each one finishes washing in the top position. Tamm has been issued a personal watering-pot; he holds it between his knees and regulates the stream by how he is squatting. Today was a day of surveying. We screened the rock through the binoculars, and climbed around the ribs and couloirs in search of a better way to the ridge. Went to the frontier guards for telephone equipment. Made a visual check of the wall height and the distance to the cave mouth. Some 800 feet, half the distance that the army climbers had given. Everybody is coughing from the low humidity in the Eastern Pamirs (10-20 per cent instead of the customary 50-60 per cent)."

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[5] N. Kastere. *Ten Years Under the Earth.* M., 1956 (in Russian).

"July 21. Not a speck of snow on the rocks of the ridge. So we'll have to carry drinking water, too. Nikolai and Lyosha started off on the first test-climb to the ridge. After lunch their tiny figures stood out against the blue sky. Tamm and I were waiting for them under the northern wall and signalled to them when to stop, when they were right above the cave mouth. From there they began to lower a little red bag with stones in it to mark the exact route for descending to the cave. Nothing doing. The bag kept getting stuck on a ledge 40 or 50 yards down. It must have been this ledge that caught the rope the army climbers threw over. We will clearly be making our attempt from that ledge."

"July 22. Those who've been up the ridge are sure it is at least an ascent of grade 4 (5b being the highest — *Ed.*). Lyosha got very cold on the way up and is running a temperature. Today we tested the equipment and trained the descent. A double-descent with a block-brake was successful; we tested ascents and descents on the rope-ladder."

"July 23. The plan for the assault is ready. First, we take up 450 pounds, then six of us climb to the ridge while Tamm and Yuri watch and correct the descent from below, on the North side of the wall. Descent down the wire cable using the block-brake will be from the ledge, 40 or 50 yards beneath the ridge. The first pair will be lowered down on two safety ropes and will have a telephone cable. Coming level with the cave mouth the pair throw a 'cat' in, pull themselves up into the cave, and unfasten the wire cable which is then pulled up for the second pair. The last pair use the rope to lower the wire cable and the block-brake down into the cave for the four (and the treasure) to move from the cave; then they climb from the ledge to the ridge and return home by the route by which they came. Tamm left for Chechekty."

From July 24 to 27 we took the loads up the ridge and finalised preparations.

"July 28. General ascent of the six onto the ridge. Unfortunately it is Sunday and there may be sightseers. At 12 o'clock we start off to the narrow couloir. Holds are plentiful and we decided to do without roping so as not to disturb loose stones. The ledge at last. The heavy rucksacks off our shoulders. An abrupt cry and Nikolai Nikolajevich disappears. All dash to the edge and freeze there in horror: he is falling headlong knocking against stone projections. A huge stone in the couloir arrests his fall about 50 feet down. We rush towards him. He's alive. Stirring. He is bruised but no bones are broken as far as we can tell. What an escape! Lyonya hurries down to get the stretcher ready, the lorry, the people. Boris and Lyosha firmly wedge themselves in the couloir and we load 90-kilo Nikolai Nikolajevich onto them. From

above, I attended to our casualty and Alexei to his support. Several hours later we reach the foot of the couloir. It is getting dark by the time we reach Murgab hospital. The X-rays show several cracked ribs and a fractured wrist."

"July 29. We feel depressed. Decide to look for the second cave deep inside our ravine. This has been visited by travellers several times. We searched for it till dark, but in vain. Spent the night in sleeping-bags without tent."

"July 30. We climbed around the whole slope and found the cave 300 to 400 yards up from the valley floor. Spent all day investigating it. It is not simple, it is ramified. Crept into a recess where Bleshunov found an ice-covered scarab. Got into a large grotto behind a pile of stones. Found a shaft with inscriptions containing interesting data: the total length of the cave is 700 feet. The fall from the entrance is 300 feet. There are barometric height marks of 4,400 and 4,610 (metres — *Ed.*) ..."

"August 1. Assault on Mata-Tash. The plan is the same, only five of us climb up onto the ridge. We take special care and reinforce rope safety. Move without the rucksacks in the narrow passage and on the 65 yard wall which is to take us to the ridge, and then pull up the rucksacks. We spend the night on the ridge, having tied ourselves to the hook."

"August 2. All day long we carried loads to the saddle from which we plan to begin the descent. In the daytime a white-headed bird, the famous gryphon with its six-foot wing span, flew over us. It made several circles and then flew off. We do not have to worry about it: we have protection for our heads and faces."

"August 3. We look things over. The ledge is some 40 yards below the ridge. We hammer in a hook, fix a safety-hook and pass a double rope through it for free descent down the wall. A few hours later both we and the load are on the ledge. The ledge is two to three yards wide but above and below there are smooth vertical walls. We don't risk pulling the rope out of the safety-hook — it is our only link with the world. Two small figures, Tamm and Yuri wave their hands down below. The little red bag with stones is creeping down the wall again; we check out the vertical line and distance to the mouth of the cave. Five hundred feet. We hammer in the hooks, fix the block-brake, hang the wire cable and the telephone cable reels, carefully coil up the loops of the safety rope, clear the ledge of stones. At last everything is ready, but now the sun has set. The descent will begin in the morning."

"August 4. All jobs have been assigned. Boris and Lyosha will go down. Alexei will pay out the wire cable through the block-brake. Lyonya will use one hand to pay out the telephone

cable, and the other to hold the receiver to his ear. I am to pay out the safety rope through the safety-hook; there is a mark on it every ten yards, which makes it easier to control both speed and distance.

"We start rigging things up. Boris is more experienced and will have to deal with anything unexpected. He will go down without his rucksack for operative freedom. We hang a rifle, a cartridge-belt, a dagger, a 'cat' with a hank of rope, a hammer, a roll of hooks, a carbide torch, a camera, and a pickaxe (to dig out the treasure) on to Boris in that strict order. Lyosha looks less martial but just as picturesque. His huge rucksack contains everything for spending the night in the cave. A fire-helmet on his head, a telephone receiver on his chest. Both are wrapped in parachute slings. We check for the last time that everything is properly fixed. Tartaren is nothing to our heroes.

"Everyone to his place. Boris and Lyosha lie down on their bellies and let down their legs. The descent begins. We pay out the wire cable, rope and telephone cable slowly and evenly ... about 10 feet a minute; the unreeling of the cable and the telephone wire slows down the whole process. After about 20 minutes the descent is interrupted — the telephone wire reel gets fouled. Hurriedly and with shaking hands Lyonya tries to disentangle the coils. Nothing doing. Minutes are passing by, nerves are on end. Lyosha's voice in the receiver:

" 'Let us continue down, don't keep us back, I can't hang here with the rucksack, it turns me over...'

"We cut the wire, take several tangled coils off the reel and splice it. The descent goes on. But two or three minutes later the whole thing repeats itself. How can a sealed new reel have tangled coils? Again we cut it and splice it. But after a few minutes it happens again. The two below are getting nervous. It's been more than 40 minutes and Lyosha is keeping upright with great difficulty.

" 'Cut the wire,' Boris shouts down the line. 'Communication will be via Tamm.'

"We cut the wire and the descent goes on. The gryphon appears from behind the ridge. It is gliding effortlessly in smooth circles towards the cave. A dull shadow sweeps over our heads. The air filters through the tips of the huge, spread wings with a vibrating hiss.

" 'Will it get bold enough to attack people singly?'

"Information from below:

" 'The gryphon has landed in the cave.'

"The descent goes on. Rumours that we were to attempt the

cave must have reached half the Pamirs, for quite a crowd have gathered below. Lyonya is watching them through the binoculars:

" 'They sit there in groups, drinking and eating. The frontier guards have just cut up a water-melon,' Lyonya says wistfully.

" 'Hey, you up there,' Yuri shouts up. 'Pay out faster, the cable is twisting, it's hard for the boys.'

"We pay out faster. 140 yards. That means they will soon reach the cave.

" 'The gryphon has gone.'

" 'Thank goodness, one trouble less.'

"Why doesn't Boris tell us to stop lowering then? They should be level with the cave by now. We are puzzled.

"Finally comes the awaited signal:

" 'Stop. Fix the cable. We're here.'

"One hundred and sixty yards in one hour and ten minutes. We sigh with relief. Not much left to do: some 6—8 yards up the stonework and they are in the cave.

"We thought we could relax, being confident of our success. But 30, 40, 50 minutes pass and no news. Strange, from below the climb up the stonework didn't seem difficult. An hour passes.

" 'Attention, up there. Boris cannot climb up to the cave, he is afraid the stonework might collapse. He is asking to be lowered all the way down to the ground.'

"Down past the cave? After so many dreams, preparations, efforts, hardships, endless carrying of heavy rucksacks up onto the ridge? We are stupefied, discouraged. Can't believe in the necessity of retreat when the hardest part is behind. And what about all those people sitting under the wall and all those people in various towns waiting for the news on the pages of *Komsomolskaya pravda?*

" 'Start lowering me, boys. I can't try again.'

"Gradually we realise that insisting is out of the question. Climbing up the stonework can't be done. We can't risk a human life. So we have failed. Disappointing for all the people who have been waiting to find out the truth about the legend of the Mata-Tash cave. We silently pay out the whole length of the cable.

" 'They are down. They are just unfastening themselves from the cable.'

"The day is ending. Cars with disappointed sightseers leave one after another. The two have completed the descent of the lower wall and all four have left for the camp. The area is empty now and back to its usual calm. We eat and get into our sleeping-bags. How will we climb up the wall tomorrow to get onto the ridge? Lucky we didn't pull the rope out of the hook.

"Foot by foot, minute by minute we scale the wall of rock. Three hours later we are on the ridge, and eight hours later we're down in the valley. Three figures come to meet us. Why three, not four?

" 'How good it is to be together again and not to worry about you all!'

"Poor Tamm, he was more hollow-cheeked than any of us.

"Later we learn the whole story of the descent and the reason for Boris's absence from the camp. The descent on the cable turned out to be very difficult. The inward angle of the wall soon made it impossible to touch it and it became increasingly difficult to stay right side up. Things became worse because of the twisting cable. When at last they got level with the cave, Boris tried to throw in the 'cat' but could not get it to catch. Then they decided to go down to the stonework and to climb up it to the cave mouth. But the stones were loose and Boris was afraid the whole lot would collapse. Yet that was probably not the main reason for their failure. When the two of them got down to the ground, Boris could hardly stand and his temperature was 105°. He had a nasty bout of tonsillitis and had to be taken to hospital. He confessed to having felt bad before the descent but had hoped to get by and had not said anything, made the more timid by my telling him off for eating ice and snow. Exhausted by the strain and by his illness, Boris had lost confidence in himself and failed to cope with the final barrier."

From August 6 to 10, while our patients were recovering, we investigated the far cave again and found a touch-stone, a talisman and a metal buckle covered with red varnish.[6]

On August 11 we left the Pamirs.

* * *

After the Pamirs Tamm and I paid several visits to other mountain ranges in the same groups. In the summer of 1959, we joined a team making their way from Alma-Ata to Issyk-Kul Lake via the Kungei-Alatau Ridge; in 1961 we went round Khev-

[6] In Moscow, archaeologist A. N. Zelinsky was kind enough to "read" our finds. The touch-stone and the talisman he dated at 6th-5th centuries B. C. in the Saks period of culture. The metal horse harness buckle relates to later Saks culture — the 2nd-1st centuries B. C. These finds extended northward on the archaeological map of the Pamirs, the area to which Saks culture spread.

suretia; in the winter of 1963 we skied the northern and southern slopes of the Carpathians; a year later we visited the Dombai Glade and Alibek. Tamm really enjoyed it and was fond of recalling all the adventures and funny situations that we got into.

He found meeting new people a great pleasure, especially if their professional sphere was different from his own. But he could never bear obtrusiveness, even if it was well-intentioned. I remember our group fleeing in the night from a Kutaisi hotel: we could not cope with the insistent local hospitality.

Tamm had a very particular and charming way of saying "Good morning". In a camp conversations usually begin from the moment you open your eyes. But Tamm had a ritual of his own. He got up, stripped to the waist and washed in cold water; then he dressed, came over, stood to attention and meaningfully said, in a kindly voice: "Good morning, Elena Alexeevna". It was just impossible not to feel moved.

He always carried an English or a German novel in his rucksack. In the Pamirs, for example, he used to read Feuchtwanger every night by the light of a tiny torch and in the morning he told us what he had been reading. Even during his holidays Tamm only interrupted his research work for a few days. And as he usually carried nothing with him for the purpose except a pen and a notebook, he derived the needed formulae anew. When it was your turn on kitchen duty it was a pleasure to take him his meal: his appetite was excellent; he ate with gusto and delicately; he washed his cups and plates himself and he never forgot to thank the cook.

Tamm always did his full share of the chores in camp and hated being shielded. Any attempt to spare him some hard or unpleasant work usually ended up with him doing twice his stint. There was only one thing Tamm and I regularly argued about. In the mountains I always liked to walk in the rear so that I could see what was happening. But Tamm saw it as an attempt on my part to watch how he climbed and got angry and would complain. Once we found a horse for Tamm to ride. But he refused to mount it and led it by the halter all the way, so that he got more tired than we who had no horses.

I wouldn't like my piece about Tamm to sound cloyingly sweet. He just wasn't that kind of person at all. But it was so fine and pleasant to be in his company.

D. A. Kirzhnits

A FEW TOUCHES TO THE PORTRAIT

Here I'd like to relate one small episode in Tamm's life which demonstrates his most distinctive quality, a willingness to put himself out for people, to give up his time, energy and comforts to help someone.

It is hard to remember when I first learnt about the physicist Tamm. His became a household name soon after the War when with S. I. Vavilov, I. M. Frank and P. A. Cherenkov he was awarded the State Prize for the discovery and explanation of Vavilov-Cherenkov radiation. I remember very well how a group of us, second-year students, "experts" in the theory of relativity, were amazed to learn about "superlight" electrons. Of course, we soon caught the drift of the Tamm-Frank theory and its beauty. Later we studied electrodynamics to Tamm's famous course and did justice to its clarity and profundity.

It is inevitable that Tamm appeared to us some kind of patriarch. To meet him, talk to him, know his mind seemed well beyond our reach. However, once in the late 1940s my supervisor, A. S. Kompaneyets, said to me, "It would be worth discussing this with Tamm. Here's his phone number, ring him up and arrange to see him". I expressed doubt that Tamm would meet me, but Kompaneyets answered: "He is not that sort of chap".

Just how right this was I saw upon my arrival at Tamm's apartment in Chkalov Street where right away I had to stop him helping me off with my coat. Some might consider this banal, but from the very first minute of our acquaintance I was captivated with his simplicity and friendliness. The fact that I was thirty years his junior made no difference at all. I think everyone whom he ever happened to meet has the same impression.

We talked mainly of science, and a bit about life in general. I was then engaged with the urgent problem of vacuum polarisation and in particular was trying to account for the results of the experiments on the Lamb shift and anomalous electron magnetic moment through corresponding non-linear effects. One of the questions which I put to Tamm and which was bothering me at that time was that there was an obvious disagreement with the gauge invariance in the theory of vacuum polarisation. He answered that as far as he could remember, something similar to my observations had been suggested in an article on a magnetic

monopole by Dirac. This reference proved of considerable value to me in my subsequent research.

I also proudly told Tamm about my "proof" of the impossibility of developing quantum electrodynamics into a perturbation theory series. This statement, I thought, followed from the Thomas-Fermi method when applied to the vacuum polarisation phenomenon. Tamm criticised my conclusion right away, explained it as a methodical effect, and this was to turn out to be correct. However, he did it in such an inoffensive and friendly manner that I left feeling encouraged.

I visited Tamm several times more and when I graduated from Moscow University, he tried — but failed — to get me a job at the Lebedev Institute; his later efforts were also unsuccessful. Finally, I had to go to Gorky where I was given a job as a factory engineer. I left Moscow with a memory of a simple, kind and wise man, ever ready to give advice or support.

My work at the factory, especially the first while, bore little relation to theoretical physics. I recall my somewhat ludicrous start. The senior officials had no idea at all of what to do with a theoretical physicist, and with little hope they asked me on what subject I had written my diploma work. When I said "Vacuum polarisation", they were delighted. "That's good. That's exactly what we need!" they cried and I was given a soapdish and a shaving brush and was sent off to find the leak in the vacuum unit.

In the following years my main worry was not to lose touch with science. Eventually I managed to create something which I ventured to submit to the experts. I sent it to Tamm. I did so without any hesitation, although in the preceding years I had been in closer touch with other leading theorists.

Some time later I received a letter from Tamm in which he wrote that my results had been reviewed at a Lebedev Institute Theoretical Department seminar (my paper was presented by V. P. Silin), that it had aroused interest and would be published. The letter ended in a way that made my heart leap: "I've taken certain steps with the administration to have you transferred here; they are favourably disposed, though we'll have to wait. I hope for the best." At that time I was completely unaware of many other things that had happened: for a long time Tamm had been occupied with very significant applied research and he had only returned to Moscow shortly before my letter arrived; his merits had been officially recognised in the title of the Hero of Socialist Labour and other decorations; he had managed to get several theoretical physicists transferred to the Lebedev Institute from other places; and that soon the Presidium of the USSR Academy of Sciences would approve his memorandum "The Cardinal Problems in Theo-

retical Physics" where he pointed out the waste and inefficiency of employing young theoretical physicists as engineers and school teachers.

However, the business of "extracting" me from the factory (as Tamm put it) took several more months and a great deal of effort and persistence. Finally, the fairy-tale ending arrived in a letter from Tamm. I still remember it by heart, "Dear D. A., I have just received reliable information that N. has given the go-ahead for you to be transferred to the Lebedev Institute. Very glad both for you and for us. Yours I. Tamm."

Some time passed before I stood in front of Tamm in the same apartment in Chkalov Street, papers in pocket and a feeling that it could not really be true. All my words of gratitude were cut off right away, and my host asked me what I was planning to do at the Institute. I asked instead for his advice on how to make up for what I had missed while I was away. Tamm had two suggestions: either to do it in a systematic way or take on some specific research and catch up as I went along. He was in favour of the second alternative and suggested that I join the work on the development of the Tamm-Dankov method he was involved in at the time.[1] I have to admit that although I laboured diligently for several months, my efforts produced nothing but a small article of minor significance. And yet I have fond memories of that time for it brought one of many lessons for living that Tamm gave me during the twenty years I knew him.

A person must be satisfied by his work, and my working on the Tamm-Dankov method was a source of negative emotions. Therefore, alongside this work I secretly continued my research on generalisation of the Thomas — Fermi model which I had begun in Gorky. Eventually, I made up my mind to confess Tamm of my "second life" and to show him my results.

I expected that having been transferred to the Institute with such difficulty and not having lived up to their hopes, and, to top it all, engaging in some irrelevant research, I would be given a good dressing down. It seemed inevitable. However, to my amazement and relief Tamm understood everything from my very first words. He became excited, plied me with questions, and after my report at the seminar I received instructions to concentrate on the new subject and to see it through.

All I have related above has had a great impact on my way of life. Yet it is just a fraction of the feelings and experiences my memory associates with Tamm. Throughout the years I was in

[1] See also the piece by V. Y. Fainberg in this book.

close contact with him I came to know other aspects of his personality and about other periods of his life. With pluck, one might even try to give a more general view of Tamm, for which one would have to go beyond the realm of hard facts. But others, I feel, are better qualified than I for that.

V. D. Konen

THREE EPISODES

I would like to tell you three episodes that revealed features in Tamm's personality hitherto unknown to me and perhaps to many others as well.

* * *

Shortly before the War one of Tamm's students gave a party after defending his dissertation and gaining his degree. The talk drifted into a rather superficial discussion of Dostoyevsky, the kind of small talk that is common at such parties. Tamm, the new degree holder's mentor, was the only person over forty. But, as might well be expected, Tamm was the heart and soul of things. A peal of laughter followed something he said. As soon as the conversation changed, Tamm turned to me and said very quietly: "I cannot read Dostoyevsky. Each line of his gives me so much pain and suffering that it leaves me feeling ill." He was silent for some time and then added: "That probably means that I get his message."

Another episode. We had known each other for nearly twenty years, and I firmly believed that Tamm and music were absolute strangers. He himself was certain of it and used to recall laughingly how futile his piano lessons had been in his childhood. He did not recognise a piece of music he had been working on for a year when it was played by somebody else. That was what delivered him from piano lessons which were in those years the bane of his life. Only then did his mother realise that it was hopeless keeping up his music lessons. So naturally enough we never discussed music in his presence.

Once when Tamm came home on a short visit (he was working outside Moscow at the time), he wanted to call on us on his only free day. It so happened that we had a musical soiree

that day. Natalia Tamm, his wife, a genuine music lover, was very glad to come and Tamm was determined to stand up manfully to the trial of keeping silent in company for upwards of an hour (which in itself was very much against his nature) and listening to sounds produced on a piano. My partner and I played variations on the "Death and the Maiden" theme from Schubert's D-minor Quartet and the first part of Beethoven's Ninth Symphony arranged as a piano duet.

As soon as the music was over, Tamm came to life and began to talk very fast and with great conviction sounding as if surprised himself at what he was saying. I was struck by the accuracy of his characterisation of the music we had played. It was all the more surprising that he, most probably, had listened to it for the first time in his life. One might have thought that he was privy to the principles of various musical styles. I recall that he noted the absence of any inner movement in Schubert's piece and the predominance of natural beauty in details. Tamm said this had not really touched him. But the Allegro of the Ninth Symphony — one of the most complicated of all symphonic compositions had had a shattering effect upon him. He pointed to the powerful inner pulse, the clearly perceptible logic of the movement, spoke of the integrity of the whole which was the more striking because of the gigantic scope of the piece. It was these features of Beethoven's music, he stressed, that had affected him so deeply. He himself seemed astonished at the impact of the music upon him.

On his next visit to Moscow we invited Tamm (not without a touch of humour) to listen to music again. He attuned to our mood and promised, though not quite sure of himself, to have a try. A recording of Shostakovich's "Selections from Jewish Folk Music" was just out, and we put the disc on. The first strains of the melody saw the smile wiped off Tamm's face and replaced by deep concentration. Having listened to two or three of the songs, he wanted to see the music so as to follow the text better. Tamm spread the sheets on his lap and sat motionless until the whole record had been played. He simply said he liked the music and named a few songs he wanted to listen to again. All were tragic songs. There was "Winter", the most penetrating song of human sufferings and, I would say, of hopelessness I have ever heard.

But before long the mood induced by the music changed. And Tamm, as usual, was talking, laughing in his own contagious manner, telling us his endless stories and giving the impression that he was a man immune to the darker sides of life.

V. A. Krainin

ABILITY TO BE AMAZED

He would not so much enter as burst into our laboratory — a short man, agile as quicksilver, the look in his eyes kind and very attentive, and immediately inquire in his characteristic gabble:

"Well, what's new?"

It was 1943 and we were in Kazan. Most of the research establishments of the USSR Academy of Sciences had been evacuated from Moscow and Leningrad to Kazan State University. Our Machine-Tool Engineering Institute X-ray laboratory was given a large half-dark room in an annex in which in the 19th century N. I. Lobachevsky, the great Russian mathematician, had had his study.

From the beginning of 1943 we X-rayed phenomena occurring in HE detonations. Tamm was sincerely intrigued by everything he saw: the kenotron with short-time overheating cathode that served us instead of the usual pulse tube; our method of synchronising the X-ray flash with the explosion phase under investigation; and, most of all, in the results of our radiographic experiments.

It was a time of puppy-love for the new research techniques. The results of the experiments were often not too strictly checked which meant conjecture was "overthrowing" well-established views right, left and centre. The following incident comes to mind. We were X-raying a small cylinder-shaped lead azide charge. Lead azide is a widely used HE and we chose it because lead vapours are clearly seen in radiographs in contrast with other products. A surprising radiograph was obtained in August, 1943. We expected to see a light field close to the axis of the charge and fields of a darker shade on the periphery of the explosion cloud. Instead we saw a reversed picture: the darker field was inside, while an edging of a lighter shade was in plain view all round the cloud.

Tamm asked us:

"How can it happen? The density at the centre of the cloud should always be greater than at the edge. Something must have gone wrong here with your experiment."

For a year we racked our brains and others' to make some sense out of what we saw. Then a very simple explanation was found. It turned out that after an explosion the aperture in

a piece of charge-holding cardboard looked in radiographs surprisingly similar to the bright edging of the so-called higher density zone. A check experiment was staged: instead of placing the charge on a piece of cardboard it was suspended with thin threads from the frame. The "higher density" effect along the perimeter disappeared. In the previous experiments the edges of the hole punctured in the cardboard by the explosion had captured the lead vapours creating thereby the "higher density" illusion. Tamm was delighted that we had found such a simple explanation.

Then we commenced a series of experiments on shaped charges. Tamm would discuss with us the radiography methods and the intermediate results obtained; he helped us find the "geodesic curve" that presented maximum information with a minimum of experiments. Though he had devoted his whole adult life to theoretical physics, Tamm together with us was mastering the physics of explosions and doing it with great flair. His hints were largely instrumental in revealing the mechanics of hollow-charge projectiles — a "mystery" at that time. By mid-1944, we succeeded in showing the effect the hollow-charge recess cover had upon the performance of such shells.

After the War we were to meet again at the very beginning of 1950. Tamm was leading a team of young physicists looking into thermonuclear fusion. Without hesitation Tamm had given up his theoretical physics research and switched his attention to applied matters urgent for the nation.

We carried on our radiographic and other investigations into HE detonations. And, as in 1943, we were astounded at Tamm's ability to get into matters quite remote from his own direct pursuits. In 1948 and 1949 our laboratory discovered that explosion products and dielectrics explosed to powerful shock compressions possess increased electric conductivity. The resistance of explosion products close to the detonation wave front was, as it appeared, smaller by several orders of magnitude than prominent theoreticians had thought. This was of considerable practical importance, and everyone got rather het up. Tamm was active in backing us experimenters. An encouraging testimonial he wrote in 1950 is a cherished possession of our laboratory.

Tamm was intolerant of any breach of ethics by scientists. The older staff members of Tamm's Theoretical Department of the Lebedev Institute say that throughout the fifty-odd years the Department has been doing research, not a single conflict involving priority, authorship or other kindred clashes of interests has ever occurred. By 1970 the Department had over

40 researchers, each creatively independent in his own way, but the climate cultured by the Department head excluded the intrigues and conflicts that can have such an adverse effect upon the output even of small research teams.

That such a conducive atmosphere existed is not to be ascribed to Tamm as an administrator. The authority he enjoyed, and his excellent command of diverse aspects of theoretical and experimental physics, were, to be sure, of no minor significance. But I would attribute this climate rather to Tamm's remarkable propensity to treat all honest people with genuine respect. His admiration for others was extraordinary embracing almost everybody — excepting only those who preached immoral and pseudo-scientific ideas. That kind he abhorred.

Once Tamm gave a lesson on ethics to yours truly. The exact date my diary tells me was May 15, 1952. The conference with the scientific leader of the Institute that day was noisy. A large group of staff members had just been awarded State Prizes and decorations. Several main developers and designers of a new instrument had been "dropped" from the award list. I delivered a highly emotional, and not too well ruminated speech. On the one hand, I quoted Pierre Curie as saying that "Scientists need laboratories, not decorations". On the other, I stressed the injustice of leaving some members of the project out of the award list (naturally, I did not mean myself).

The conference over, Tamm saw me home, saying:

"In principle you are right but you must always fully realise the objective of each public statement. Do you not feel that what you said today is not quite ethical in relation to our scientific leader, whom you respect so much?"

The conversation lasted some 30 minutes, and I shall remember it as long as I live.

Tamm had a blend of kindness, concern for the needs of others regardless of their social standing, and high moral principles. From 1950 to 1953 we often had to take flights in LI-2 planes in which the passenger area was not heated. It was all right in summer, but in winter travelling was far from pleasant. Doctor P. recently recalled the following incident. In December, 1951 Dr. P. was accompanying a sick three-year-old child on one such flight. Tamm and his wife Natalia Vasilievna were among the passengers. On seeing the child in the doctor's arms, Tamm grew agitated:

"He'll be frozen stiff by the time we reach Moscow."

The Tamms got some woollens out of their suitcases, but even that Tamm thought not enough: he went to the captain's

cabin and only settled in his own seat when the sick child had been placed in the radio-operator's heated compartment.

There are people who do a good turn counting on getting something back. And then there are those who derive pleasure from doing good which realisation raises their own self-esteem. Tamm belonged to a superior brand of human beings who do good quite naturally without thought for themselves. His was a humanity resting squarely upon principles and profound convictions. A humaneness that gave no consideration to things temporal whatever benefit they could bring him.

Like all natural scientists of materialist philosophy he was in raptures over the discovery by Crick and Watson of the structure of the gene as the heredity material medium. In a series of brilliant lectures read from 1956 to 1964 he popularised the "dual helix" and the "heredity code". He drew large audiences from people in a variety of callings. Under Tamm's influence I. V. Kurchatov organised a biological sub-division at the Atomic Energy Institute, Tamm taking an active part in its research up to 1967.

Here are two more examples of Tamm's ethics and his intolerance of unjust, arbitrary actions.

In 1952 he learnt that a physicist had come out in support of a group of philosophers accusing Albert Einstein of being a follower of Mach and an idealist. The article, in which the physicist in question expounded the existence of an absolute system of co-ordinates, was read out in a friend's flat. In great annoyance Tamm got up, moved his chair aside and said:

"His echoing those philosophers, whose understanding of the theory of relativity is nil, is bad enough. But what's really repulsive is that the fellow writes up things that he does not believe in himself."

Some time later there was a vacant seat next to Tamm at some meeting. The physicist referred to above was late, took that seat and greeted Tamm. Tamm got up and moved to another seat.

About the same time a mathematician was dismissed from the Institute for reasons somewhat obscure. When the news got around, some of the staff, though knowing their colleague to be innocent of any misdemeanour, pretended not to notice the fellow when they ran into him. Not so with Tamm: on the mathematician's last day at the Institute Tamm told his colleagues: "I shall not be here after lunch: I'm going down to help him pack his books and things."

Tamm's sixtieth birthday in 1955 was marked in a very modest way. The scientific leader of the Institute had a fancy

cake baked and iced with: "I. E. Tamm — 30 years." In making the presentation he said:

"Igor Evgenievich, I knew your birthday was coming round. But your age, I'm afraid, escaped me. 'How old is Igor Tamm?' I asked the theoreticians. 'Wait a bit and we'll work it out,' said they. As usual they 'lost factor 2' in their estimates. Alas, too late. The inscription had already been done. But I have got to thinking that the text is right after all. Sixty years, indeed! We'll never credit that you are over thirty."

And that was quite true. An incident on an angling trip is still fresh in the mind of many. One Sunday a group of research fellows of the Institute, Tamm among them, set out to the country. The wife of one of the party asked her husband to let her cast his spinner. And that she literally did — down into the water it went, the spoon-bait and the line, and the reel and rod as well... A family scene was brewing. Tamm disappeared into the bushes, undressed and in a few minutes was diving like a twelve-year-old. The ill-fated spinner was recovered and returned to its owner.

Psychologists assert that an ability to be amazed at things is one of the main indications of youth physically and mentally. Tamm never lost this ability. He had it to his dying day. At the beginning of July, 1967 my wife and I visited him at his country cottage near Moscow. He was playing chess, but put the game off upon our arrival. We got to talking on a host of things: genetics, technology, physics and medicine. I told him of new ideas and practices in treating schizophrenia. His wonderment was sincere:

"So, before long, people will get the better of that terrible affliction! Is that not wonderful? But is that really possible?"

We met for the last time in his Moscow flat on Maxim Gorky Embankment on November 26th, 1968. His wife, Natalia Vasilievna, asked us to talk to him for an hour, no more. But he wouldn't let us go for more than two hours.

I showed him the first miniature dry battery-powered X-ray cameras, electronic hearing aids, recounted what wonderful properties 55-iron had, and demonstrated X-ray cameras and other devices based on the radio isotope. His voice was subdued, but for the rest Tamm was his usual, amazable and amazing self. Those were very hard times for him, but his thirst for the new was as unquenchable as ever. He would not let us go:

"Oh, do stay and tell me something more."

At the end of our visit he said quite unexpectedly:

"You are a happy man... Only a man really satisfied with

what he is doing can speak with such passion and abandon about his instruments and his ideas."

And when we were taking our leave he repeated:

"What a happy man you are!"

I have recalled many times those parting words I heard from Tamm. Meningitis left our eldest daughter stone deaf at the age of nine. In 1966 we buried a son not yet 18. I have almost completely lost my eyesight. Tamm was fully aware of the calamities that had befallen our family. But, taken by itself, the concept of human happiness rises above all grief caused by illness and the negative emotions that abound in our lives. The joy of creativity, of triumph in struggle against heavy odds — are these not the main components of happiness?

Was Igor Tamm a happy man himself? He did not live to see the coming of controlled nuclear fusion, something he worked on with great enthusiasm in the first half of the fifties. Nor did he witness the arrival of the general theory for the structure of microcosm he dreamt of in the last years of his life. And still he was a happy person! And not merely because he had expounded the Beta-forces theory, explained the Cherenkov effect, or founded in Moscow a school of theoretical physics of world renown. High civic awareness and principle in all situations, integrity, kindness and concern for others were his features, features that like a magnet drew to him all those who had had the good fortune to speak to him even once, or attend his brilliant seminars and lectures.

There are recesses in the human memory for storing most important and cherished events. My acquaintance and contacts with Igor Tamm, a charming man who lived one big happy life, belong right there.

B. G. Kuznetsov

THINGS PERSONAL AND EXTRA-PERSONAL

For many years I met Tamm only rarely, mostly on ski runs and in the swimming pool. Only during the last decade of his life did our meetings become long and frequent. But even earlier each of our dialogues had given me much food for thought and could not be easily forgotten. Tamm did not like to speak about himself. But ideas stimulated by our multi-thematical

conversations would make me think about him. About him and science, its subject matter and ideals. That was the main impression derived from our meetings and exchanges: any topic would set off a train of thoughts about the personality of my companion and at the same time about quite impersonal things. I can still hear Tamm's voice. I reproduce his remarks inside my mind and ponder over them. The desire is ripe within me to share the thoughts and feelings such memories evoke in me with others and it is not easy to find the right words to commit them all to paper. These reminiscences and ideas merge naturally with inferences on modern science, the modern thinker and the links between the scientist and the subject matter, on the style of non-classical physics and its emotional accompaniment.

Tamm belonged to the group of scientists whose personalities have to be studied to gauge the style and motive force to modern science. Traditional theories seem to be less intimately linked with the psychological make-up of their creators than non-classical science. At least that was the impression one got in conversation with Tamm and, especially, in talking with him about Albert Einstein. Our discussions on the subject became quite frequent at the beginning of the 1960s. In 1962 an Einstein Committee was set up at the International Congress on the History of Science. With several other of our scientists, Igor Tamm represented the USSR. Some time later the Soviet Academy of Sciences formed a National Einstein Committee, with Tamm as its Chairman.

One of the first meetings of the International Einstein Committee[1] was held in Paris and Tamm and I attended it. It was Tamm's first visit to Paris and he was awash with impressions. Everybody who has been to Paris will remember a feeling of it having a succession and continuity of cultures. We discussed that feeling while strolling in front of Notre Dame and in the public garden behind it known as the Archbishops' Garden.

The Cathedral seemed the very embodiment of historical continuity: the remainder of a pagan temple built into its foundations, its outline blending into the skyline of modern Paris, with the chimeras as symbols of the paradoxicality of modern scientific thought.

It was this paradoxicality that Tamm and I were discussing at the time. I was struck by the presence of what appeared to be two contradictory trends in Tamm's outlook. He had a profound grip (intellectual and emotional) of the striking paradoxicality

[1] B. G. Kuznetsov was the President of that body — *Ed.*

of relativistic and quantum ideas; at the same time he understood their logical and historical links with many centuries of rationalist thought and three centuries of classical science. The reference to rationalism had quite specific local overtones in a city along whose streets Descartes had walked. I talked of a certain rationalist, Cartesian tradition in the theory of relativity compared with Bohr's ideas. "But do not forget the correspondence principle," Tamm objected.

Such transitions were quite characteristic of him. Each time, as I have already mentioned, his remarks made me think about his personality. "Extra-personality" is a thinker's ability to divorce his thoughts from his own self, and to get absorbed in extra-personal ideas; it is a personal quality and the measure of the magnitude of a man of science. There was more to Igor Tamm than his humility. Applied to him, the attribute "modest" was positive. Tamm did not think about himself, and always, without intermissions and regardless of his own private life, he kept thinking about the universe. Even during his tragic illness he was contemplating *rerum natura* (the nature of things).

We recalled that title of Lucretius' poem as we sat on the stone steps of the Lutetian Arena, the Roman circus whose ruins are off Saint Jacques Street not far from the Poincaré Institute where Tamm was to read a paper on quantised space at a theoretical seminar run by Louis de Broglie. Pogrebyssky, who came to Paris with us, recited lines from *De rerum natura* from memory. Remembering his high school Latin, Tamm gave a running commentary on Lucretius' lines. A modern dictum carved in stone stood in front of us. A part of it read: "Ye who pass by, remember: here is the cradle of your city, the city of your hopes."

"Lucretius' poem is also the cradle of our hopes, hopes that a general theory of elementary particles would be created," someone noted.

Tamm followed up the remark, and our conversation drifted on to another subject — the theme of the forthcoming paper. Such transitions from historical and cultural reminiscences to forecasts in physics were quite frequent in the talks we had.

Tamm had a working command of French but was more at home with English and so at de Broglie's seminar he used English which was translated into French by Vigier. I sat next to Marie Anttoinette Tonnela, an old friend of mine, and watched the reaction of the audience. Tamm's ideas aroused general interest, and everyone seemed enchanted with the speaker's personal charm. The talk was followed by a brief discussion between Tamm and Bohm and Vigier who supported

the ideas expounded by de Broglie. As most modern physicists, Tamm was an opponent of this version, but I was interested not in the clash of ideas, but rather in the manner in which the discussion proceeded and the remarkable tolerance Tamm displayed to his opponents. This again is not a negative but a positive definition. I recalled the words Hegel used to define the tone of Plato's dialogues — "noble suavity" — implying not an external form, but a quest for something worth modifying, generalising or limiting in a wrong view, and thus bring out the expectation and desire to seek truth.

The seminar over, we were received by de Broglie who, in addition to his duke's title and reputation as a man of genius, could claim a no less honourable status as a very modest and kind man.

After the meeting of the International Einstein Committee the four of us (Tamm, Pogrebyssky, Grigoryan and I) went south touring. We took in Marseilles, Nice and the towns of Provence. We even got to the famous Tarascon where our car made a turn banned by the traffic regulations. With his usual vitality Tamm began to push the car back (rear gear was not working) while two policemen watched us with benevolent irony. "No rear gear, I see," one of them said, "so you could not serve with the Italian Army..." Wisecracks of Caporetto vintage were evidently still popular in Tarascon.

The past in general still matters in Provence. Tamm, the real mixer who could relate to other people regardless of their interests, calling in life or social standing, something true of the real thinker, spoke to many local people. He was amazed at the tenacity of the memories of the past in those parts where even now peasants find shards of Greek vases and statutes in their fields, and where one can watch a theatrical performance in a Roman amphitheatre. From time to time Tamm would hum a modern folk song he had learnt about that "Western Marseilles", or converse with us on the serious matter of continuity in culture and science. He was absolutely free of snobbery or the airs and graces some learned men put on.

Tamm returned to Moscow full of energy and vigour. "I shall not go to any spa. I'm off to a mountain camp instead," he said, discussing his plans for the coming summer. But then his health deteriorated.

I visited Tamm frequently after he fell ill. He lay on a special bed, only occasionally moving across to an armchair (an adjustable aircraft seat presented to him, if I am not mistaken, by A. N. Tupolev). Sometimes he would sit at his writing desk, but that became more and more rare. A nurse was

on duty round the clock. From time to time our conversations were interrupted by the nurse changing the position of the bed to get the patient almost upright: the iron lung called for such torturous operations.

Tamm knew that his was a terminal case. Awaiting death was a "hard experiment". But "hard" though it might be, it sometimes brings forth not contradictions between life and consciousness, but their harmony. The expectation of death can leave the soul void but it can also orient it fully towards the extra-personal, relieve it of everything transient thereby bringing into the open a human personality as an entirely individual and unique entity.

Applied to Tamm, "relieve it of everything transient" in no way implied a flagging interest in the lives of others, in everyday occurrences, or in the fate of people he knew. This was never transient to him. Tamm was attracted by life in all its facets, by the lives of all people. His remarks, now at times made only with considerable effort and pain, referred not so much to what Spinosa called "creating nature", but to "nature created". To be more exact, Igor Tamm not so much comprehended, but rather sensed the unity between Cosmos and Microcosm, that characteristic presumption of science.

I recall a chat we had to the throb of the iron lung that reminded us how swift-flowing time was. Tamm spoke on a variety of subjects and, among them, of great scientists; he dwelt not on their ideas but spoke of the small things in their and ordinary people's lives.

Niels Bohr: Tamm travelled round Denmark with him. Strangers would greet Bohr politely and respectfully. "They do it," he had explained to Tamm, "because I am the relation of a famous football player."

Dirac: Dirac and Tamm spent an evening as the guests of some family. Dirac, as usual, was silent and watched the hostess knitting. On taking leave he said: "It seems that I have discovered the finite number of knitting methods, and I can prove it."

The granddaughter of the King of Sweden: after the handing of the Nobel Prize ceremony Tamm sat next to the King's granddaughter at the Royal banquet. She was complaining that one could only go skiing in the north of Sweden; they had a small castle there, but it had to be heated well before one went there. Tamm said that his country retreat at Zhukovka near Moscow had many advantages in that respect.

He was speaking simply, showing deep concern and sympathy for others and completely oblivious of himself. Listening to

him I thought: how are these traits of character, these moral features related to his intellect, to his scientific feats; what was the common base upon which his active and creative interest in humans rested, in the particulars of their lives, in truth and the structure of the Universe?

Could one sum up in a nutshell the salient features of Tamm's character, style, interests and ethical make-up? Many words would be needed for such a purpose. One might speak here of modesty, altruism, sociability, of his empathy and openness, of the resonance joining the man's inner world with the world about him.

With Tamm all these formed an integral whole. The one feature that always leaped to the eye was his genuine concern and true respect for others — modesty's own effective equivalent. I might also say that Tamm belonged to that small group of physicists who are never in the slightest patronising to others and never make you feel that they walk on air; who never display academic snobbishness or come over as feeling superior to those working in the less exalted fields; who never make you conscious of their rank or look at you as if meaning to say: "Thank you, Lord, for not making me in the image of that publican..." But all this is a negative definition. The positive form of Tamm's nature was his empathy and responsiveness.

A few lines on his creativity would seem in place. Never has the rate of scientific development been so much dependent upon the generation of new ideas and on checking them by experiment (or, as Einstein put it, their supreme justification) as today. And upon the natural development of such ideas from more general principles (or the attainment, in Einstein's words, of inner perfection) without any *ad hoc* factors.

Thus the features of Tamm's character merge with his scientific creativity to form a picture of responsiveness. The man's humility becomes the modesty of the explorer. This humbleness rests upon an understanding of the scale of development in modern science, of what will happen to it in the future and of what has happened in science in the past.

Let us start with the past. When a research worker ignores the past, he has no modesty. Nowadays one runs across in physics, though happily not too often, a non-classical conceited supercilious approach implying that a student who has read a modern physics textbook knows more about our world than poor blokes like Aristotle and Newton. As Heine put it, a dwarf perched on the shoulder of a giant sees further. Newton indeed did not know all the physics of any standard high school textbook today. But modern science demands, beyond greater scope of knowledge,

thought spurred by the assimilation of knowledge gained in past epochs.

Heine ended off: "... but he has no beating of a giant's heart." Tamm's heart beat in unison with the gigantic heart of science in its past and its future. He felt the progress physics had made towards Aristotle's *Physics* and to presenting a physical picture of the universe as a fully integrated complex whole. Hence Tamm's interest in new ideas in elementary particle physics, and his passionate interest in discrete space problems.

Once, discussing some acknowledged achievements in physics, Tamm remarked: "Perhaps, I could have done something like that, and to some extent one feels one has done something. But the trouble is that what I can do is of less interest to me than what I as yet cannot do." Such is the lot of every scientist, who, in Ostwald's classification, falls into the category of romantic scholars (in non-classical science all those who advance it are, to some or other extent, romantics).

Time will pass, and many will have greater learning than that Igor Tamm possessed. But immortality in science does not use that yardstick. To borrow an expression of Jaures, we might say that science passes its flame on to the future, not its ashes. In Igor Tamm's heart that flame burned bright.

M. A. Markov

MANDELSTAM'S ASSISTANT PROFESSOR

Memories flow in a way of their own; they possess their own "selection principle". I knew Igor Tamm for several decades and have plenty to remember him by. Tamm's life is part of the history of development of Soviet science. This aside, Tamm was a unique personality.

But the old mind throws up something absolutely different. Trifles...

From those fargone student days in the 1920s floats up an image which it would seem holds nothing noteworthy. But, in my mind's eye it is as clear as if it had happened only yesterday: the lobby of the Institute of Physics at Moscow University and a man unknown to me. Some conflict is in progress between the man and his rubber overshoe. The stranger is hopping on one foot in hot pursuit of the overshoe and has been at it for some time; now he

catches it and is pressing it against the cloakroom counter. But the overshoe escapes, and the chase is on once more.

I watch the conflict for some time and want to say to the man to use his hands. But then it dawns upon me that the man wants to put his overshoes on that way and no other. When at long last he succeeds, he glances at us triumphantly and, slightly inclining his head to the right, moves swiftly to the exit. The old cloakroom attendant who has also been watching with interest, turns to me, raises his forefinger significantly and tells me in a confidential whisper: "Igor Evgenievich Tamm, Mandelstam's assistant."

Before long I met Tamm again. He was walking briskly towards Arbat Square. I knew he was giving lectures (in mechanics if I am not mistaken) at the Second Moscow State University as the higher school at Devichye Polye was known. Recalling the reference the old cloakroom attendant had given him and the importance he had attached to it, I looked at Tamm more closely than good manners would allow. It seemed to me that as in the Institute's lobby he flashed a bright smile at me before hurrying on. I followed the rapidly disappearing figure with my eyes and realised that he had been smiling not at me, but at some idea. And now it seemed to me that he was following that idea down the Arbat.

Years of hard work, research, hopes and disappointments flick by one after another, years of weekdays, weekends and holidays. And again some trifling memories float to the surface. It must have been just before the Second World War. The scene is Miusskaya Street in Moscow where the Lebedev Institute was at the time.

Sitting on a window-sill smoking a cigarette, Tamm was giving us a lively account of his skiing successes the previous day. He had wanted very much to race down a steep hill, but some kids kept getting in his way, it never occurring to them that the old-looking chap with bristling grey hair would venture down such a steep slope. When he was at the end of his tether, his impatience was at last noticed and somebody cried out quite loudly: "You, there! Give that wee old man a chance to kill himself!" Tamm informed us with pride that he had stood the test and had not even gone down where the young ones usually came off the rails.

Tamm was never caught off balance where morals were concerned: he was a man of unimpeachable honesty.

One always gained a lot by discussing scientific problems with Tamm. But he was a "poor" critic, or rather one who was never too hard. His amazing benevolence inhibited his critical

approach to your work. So before long he would share your view and begin to project and "build up" the idea under discussion.

"Interesting... very interesting indeed," were the words in which often (though not always) such discussions ended.

"Keep in touch and do let me know what comes of it..."

His good will was not an expression of detached politeness — it lent you strength.

Tamm worked himself with his calculations every night. Often he would come to the Institute all in and yet exuding optimism for some still incomplete project. His calculations were like his battle with the overshoes. Sometimes after efforts of many days the overshoe would just refuse to be put on. And Tamm would say: "Well, today the idea has finally gone bust." But before long a new idea would come to take its place...

It was Tamm's good luck to have a woman like Natalia at his side, a tactful, calm and discreet person. One felt that she was an indispensable complement to Tamm's personality, and that he needed her help which was more than Institute lobby sort ... Her contribution was far more weighty and deep in Igor Tamm's life. Natalia was his guardian-angel.

The memory has a way of its own and perhaps this is the logic to what has been reconstructed subjectively in these reminiscences.

F. V. Nikitin

GRAMMAR SCHOOL

Gorka Tamm (as Igor was known then) and I were at the same school in Elizavetgrad and, for some time, next-door neighbours. His father was the city engineer and ran the municipal power- and water-supply system. The family lived in a two-storey house on Petrovskaya Street with Evgeny Fyodorovich's office on the ground floor. There were three children in the family, a daughter and two sons; Igor was the eldest. Igor was on the short side. He was stocky and well-developed, had fair hair with a touch of red and a wilful face much like his father's (though the hair colour and the broad head probably came from his mother). His movements were sharp. Academically, Gorka Tamm was always at the top of his class and he always moved up with honours. Two

A high school student

incidents connected with Igor stick out in my memories of my schooldays.

Once a teacher, P. E. Brykalo, asked us each our nationality and how we came to determine it. Everything was absolutely clear until we got to Igor's case. He was sincerely at a loss: his paternal grandfather was from Thuringia, his mother an Ukrainian with some Crimean Tatar blood on one side, and a Russified Georgian family on the other... He himself had been born and baptised in Vladivostok... The class unanimously decided that Igor Tamm should regard himself as Russian, for he both spoke and thought in Russian.

The second incident happened at a Latin lesson. P. D. Dubnyakov, our Latin teacher, was a man advanced in years and a stalwart of the old brigade when it came to methods of teaching.

But Dubnyakov had a very profound knowledge of his subject. This we discovered when one day the Odessa District Inspector, Brakeneimer, paid a surprise visit during Dubnyakov's lesson. Ignoring us, the Inspector addressed our teacher in Latin. They chatted away till the bell. We looked on in stunned silence.

At half term Dubnyakov announced our marks. There were zeros, zero minuses, pluses and minuses and so on up to a few threes even[1] one of whom was Gora. But when Tamm heard that he had only got a three, he put up his hand and asked to be called to try and get a higher mark. Dubnyakov was surprised and said: "A higher mark isn't possible, Tamm has got the highest mark any pupil deserves. I myself could only expect a four in Latin, no more. And only the Lord could have a five. If Tamm believes that a three is too poor a mark for him, I can add a plus." Evidently the teachers' council straightened things out, because a few days later normal marks were put into our reports.

There was a large hall in the Tamms' flat which we used for amateur theatricals. A stage was equipped each time we put on a show — it had boards, electric footlights, a curtain etc. Kobylyansky, a retired officer, a relation of the Tamms on the maternal side, was our director. Friends of the family and school friends would attend our shows which were a success even with the adults. Igor and I were the "stars".

One unpleasant incident comes to mind. I was muscle-building at the time, liked cycling and, following Suvorov's[2] example, I slept in unheated premises, and walked out naked in sub-zero weather after hot baths to rub down in snow; I discarded overcoats and other warm clothes and wore nothing but a light jacket in winter. Gora resolved to do likewise. So he left off his overcoat and just about caught pneumonia. Having failed to talk me into changing my ways, Igor's father applied to the school principal to forbid my coming to school coatless during the cold season. The ban was imposed and had to be complied with. So, following in Suvorov's footsteps, I had to carry my coat on my arm and put it on and button it up just before I entered the school yard. When he got better, Igor gave up trying to get fit my way.

In high schools they studied physics to a course written by Kraevich; mathematics included calculus and analytic and drawing geometry, while natural science was offered in all the classes. Unlike that type of school, the physics studies at a grammar school were limited to textbook by the same author; the maths

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[1] In Russia 5 is the top mark and 3 is a bare pass — *Ed.*

[2] A famous Russian military leader (generalissimo) of the 18th Century — *Ed.*

curriculum was only arithmetics, algebra, geometry and trigonometry; and natural science was only tought in the three bottom classes. Greater emphasis was on Russian and the classic languages of Latin, Old Slavonic and Greek (this last optional), and logic, psychology and fundamentals of jurisprudence.

On the whole, the grammar school curriculum, though fairly extensive, was somewhat superficial. This was not much of a base for the natural sciences and biology courses offered at the Physics and Mathematics Department of the University, and in its natural sciences subdivision. Besides, the physics teachers had a rather formal approach to their subject, which did not get the pupils interested in physics. So, in my view, Igor's choice of physics at University was rather due to his father's influence who was an energetic man and an excellent engineer. After the Revolution he was promoted to a similar post in Kiev, where his responsibilities were considerably larger.

Finishing school in 1913, we left Elizavetgrad for higher education in different cities. In the summer of 1914 World War I broke out.

Thirty years were to pass before I met Igor again. By then he was professor of theoretical physics at Moscow University while I (with evacuation during WWII) was senior livestock consultant at the Biryulin state farm near Kazan. It was our mutual friend and classmate Professor Boris Zavadovsky who brought us together. We wrote to each other from time to time. When sent to Moscow from work, I would call on Boris in his flat near "Krasnye Vorota" metro station. Igor lived in another wing of the same building. Zavadovsky rang up the Tamms, and we went over. The reunion was touching. Igor immediately inquired: "Do you remember when we were pigeonfanciers and you won my pigeon-loft for a bet?" We exchanged memories of our schooldays. Igor Tamm married Natasha Shuiskaya, whom we both had known from our schooldays, her brother Kiryusha Shuisky having been in our class. From then on whenever I was in Moscow I used to visit the Tamms as well as Boris Zavadovsky.

V. I. Ogievetsky

"CARTHAGE MUST BE DESTROYED"

Igor Tamm was fond of quoting Cato the Elder, a Senator in Ancient Rome who ended each of his public statements in the dictum quoted above. As distinct from Cato, Tamm had more than one "Carthage" to raze. Tamm was intolerant of anything he saw as obstructing the normal progress of science. Such "Carthages" appeared often, and their destruction was imperative to erect something more efficient and useful in their place. Tamm would go into battle and in the long run he would win. Always working doggedly towards some goal he had set himself, he took an active interest in everything around him. His standards as regards his own performance were high and he was very kind-hearted. It was real joy to associate with him and it gave you a lot.

He was agile and moved in quick steps carrying on a lively discussion of this or that matter, advancing arguments and counter-arguments, trying to understand the other party and support him if there was the slightest justification. As far as science was concerned, talking to him was always both instructive and fruitful. Tamm had a gift in that no matter how stringent his criticism, discussion with him gave you new faith in your own ability. Tamm never tried to overwhelm those he talked to with his own erudition or the power of his intellect. He was always lavish with kind words.

I recall his first invitation to his home in 1949 when I was a fourth year student. He lived in Chkalov Street at the time. I related to him how I learned to simplify the process of solving differential equations by splitting the operator into a product of more simple operators. It was a mere trifle, but Tamm listened to me with rapt attention, checked my method by solving several equations and said: "You have enriched me, thank you!" And immediately offered me an interesting research theme. I was in seventh heaven.

Tamm was always helping other people. He introduced me to his students and staff members, among others to S. Z. Belenky and also to I. Y. Pomeranchuk. He always gave moral support and worked to launch me on my career in science.

Tamm was highly dissatisfied, and especially during the last years of his life, with the local quantum field theory. He objected to a good part of it, and especially the renormalisation

procedure. In 1968 he said: "A theory is unsatisfactory in fundament if it uses values which are unobservable in principle, and which then are artificially eliminated from it." His rejection of the traditional local field theory had begun a long time before, in the early 1950s, and he had been looking for new ways of developing it ever since.

In October 1955 I sent him a letter dealing with the application of Borel's method of summation to the study of series in perturbation theory. In his reply Tamm promised total support. For instance, he offered to introduce me to I. M. Gelfand. There was a postscript to the letter: "*Ceterum, censeo Carthaginem esse delendam* (All said, I think Carthage must be destroyed — *V. O.*). However, today I am still more convinced than I was then that the current theory can be used only for want of a better. We need a theory based on completely new principles."

Tamm was always very pleased when he managed to get the better of any "Carthage", no matter how small or insignificant. In 1956, if I am not mistaken, a seminar was arranged at the USSR Academy of Sciences Electro-Physical Laboratory (now the High Energy Laboratory of the Joint Institute of Nuclear Research at Dubna) which was at that time under V. I. Veksler. Tamm was invited and came to Dubna in an official limousine. There were several other passengers in the car. In the centre of the estate Tamm had offered a lift to some ten young physicists. I don't know how they had all squeezed into the car.

When the car reached the checkpoint, everyone was about to get out to show their passes. But Tamm told them to stay inside. A security guard came out and Tamm shouted something incoherent but imperative to him. The man was taken aback and let the car through complete with its overload of passengers. We were all baffled, but Tamm was as happy as a schoolboy, having got away with it! He liked to come out on top whether it was something really important or totally insignificant: treating a scientific problem, scaling a mountain, playing a game of chess or cracking a joke. The impression he produced on the security guard was for him a source of pleasure.

Here is another episode related to me by V. I. Ruskin. In the 1950s Tamm once went to Lake Issyk-Kul for his summer holidays. Tamm and Ruskin, then a young physicist, were strolling along the lakeside. Tamm was enthusiastically explaining something to his young companion when they came to a brook. Going on with his explanations Tamm took a running jump but landed slap bang in the middle. When told that there was a board footway closeby, Tamm answered: "Young man, do you really think

that I did not see it? I have already made several jumps here. I simply *must* jump this stream!"

Tamm more than once had to overcome obstacles to drawing into research work young physicists who, in his view, showed promise as scientists. Tamm put much passion, energy and time into such efforts. And he felt very much put out when he suffered inevitable temporary setbacks. As to his ability to give moral support, well it was just immeasurable! The very thought of a coming meeting with Tamm cheered one up and encouraged one immensely.

My memories of Igor Tamm are all bright and joyful and they help me tremendously in moments of adversity.

R. E. Peierls

RECOLLECTIONS

My recollections of Igor Evgenievich Tamm are vivid, but not extensive, since I never worked with him, or spent much time in his company.

The first thing one noticed about him was his liveliness and agility. This included his quick reaction and lively interest in any problem in physics as well as outside, his quick understanding of what one was trying to say, and even physically his restless and rapid movement.

I cherish a photograph taken while he lectured at the Odessa physics conference in 1930, where I met him for the first time. I did not have a very powerful camera, and while there was enough light to show all other faces in the room in sharp focus, Tamm is only a vague blur.

But, as one soon discovered, his speed was no sign of superficiality. He had a very deep understanding of physics, both old and new. At a time when many young physicists, full of enthusiasm for the new quantum mechanics, felt that anything done before 1926 was old-fashioned and useless, he knew how to combine a fluent mastery of the new method with a sound appreciation of the old, and one could always learn from him about the connection.

In many later encounters I learned to appreciate his deep understanding of human affairs, to which he brought the same modesty and humility with which he approached a problem

in physics. Once he had formed a conviction about what was right and what was wrong, he acted on it with as little hesitation as he would have about a physical truth. This emerged also in the Pugwash Conferences, where our common concern was with the preservation of peace and the survival of mankind. At these meetings his presence was always an inspiration.

I visited him for the last time in the summer of 1969, when his health was already severely affected by his illness, and he was very uncomfortable. His movements were now slow, but his mind moved as rapidly as ever. In spite of his troubles, about which it would never occur to him to complain, he was working hard on a calculation relating to some new idea in fundamental physics.

The world would be a better place if there were more people like Igor Evgenievich.

L. V. Pariyskaya

OUR IGOR EVGENIEVICH

Whenever anybody called Igor Tamm his or her teacher, something inside me always rebelled. Igor Tamm a teacher, a tutor? It was just not him. We knew each other for 45 years, 25 of which I worked on his staff. My memories of him are endless. They are associated with the Institute we worked at, excursions and boating expeditions during May holidays, innumerable Sunday walks ending in volleyball matches in forest clearings, come rain, mud and even snow. In all those memories I do not recall him ever, not even once, acting the don or the mentor. Tamm could be argumentative, indignant, angry, protesting, but he never abused his seniority, his rank or status.

At the same time Tamm was always ready and willing to learn something from others. He learnt "in a big way" from his students: listening to his young student V. Y. Fainberg talking at the seminar on a new formulation of Feynman's quantum electrodynamics, he was enraptured and called upon all of staff members to master the new method. Tamm was the first to pass a test on the subject with Fainberg as his examiner, and he took it genuinely at that.

He was always ready to learn anything big or small from anybody. While skiing, he saw a boy jump in the air and make a 180-degree turn. Tamm immediately wanted to do the same and

jumped again and again until he did it. He learnt to do a headstand at the age of sixty, and would jump over water-filled ditches...

I remember our visit to Mount Altai in 1926. Tamm called it his baptism of fire for it was there on the Belukha Glacier that he became a mountaineering addict. I have a detailed diary N. N. Pariysky kept. And in my mind's eye I see the Altai of those years, a backwater of Cossack villages and old believers' hideout farmsteads, Kirghiz nomads with huge flocks of sheep grazing on the mountain slopes, and the spritely Igor Tamm.

Did he look young? Somehow that escapes my memory. In similar situations twenty or thirty years later, he would, it seems to me, have looked and acted just the same. All people change with time, and sometimes beyond recognition, but Tamm managed to keep one image throughout his life.

When Tamm's 70th birthday came round I had to approach quite a few people with requests they might hardly have expected from me. I was most pleasantly struck by there not being a single refusal. Each time the response was surprisingly similar:

"For Igor Tamm? No problem at all! Rest assured, I'll do my utmost!"

So how had he earned such affection?

Once he told us:

"Old men fall into two groups: some withdraw into themselves in old age, others overflow their banks. I'm the second kind."

But this was not true. He never became an "old man" at all. He stayed young till the last. And that was exactly what drew people.

Tamm had a wonderful capacity to rest in a remarkably merry and easy way (a rarity now even among young scientists). He enthusiastically played any game, liked chess, went in with fervour for various word and literary games. But his favourite was charades. These were staged with special vigour when our children got to university age and two generations took part. Tamm never produced or directed the charades himself, though he never refused to act in them when he was needed. What he liked most was to be a spectator, and his enthusiasm for the entertainment was such that it charged the whole cast. We had some real acting talent among the younger people: the insolent waif in a railroad carriage played by his son young Evgeny Tamm still lives in my memory.

I remember Tamm remarking to G. S. Landsberg when we were driving somewhere:

"For me no theatre can compare with charades. Only a rough

outline of the action is offered beforehand, all the rest is improvisation! That is a real *commedia dell'arte*! You must come and see it for yourself!"

Today many research workers try to insulate themselves from "surplus" information in one way or another. But Tamm was always open to any new ideas. Everything new was a great attraction to him. Anybody obsessed with some idea went to see Tamm and found a most sympathetic listener, literally anybody with any subject! During our Sunday outings Tamm would share the bountiful news he had stored up during the week: telepathy, hidden treasures in caves, the mysteries of the hills in the forests around his summer cottage, hills that "had to be dug up".

Tamm also had more serious hobbies. Besides his immediate work, he spent much time and energy on biology, biophysics, and especially on genetics. He was ready to move heaven and earth to help genetics. He was the first to report on Crick and Watson's discovery of the DNA double helix. His lecture on the subject gathered a very large audience of physicists and biologists at Moscow's Research Institute for Physical Problems.

Natalia Vasilievna once said: "He's my most naughty boy."

And I knew what she meant. He would take terrible tumbles when skiing. Once he careered into a tree-stump and a huge haemorrhage formed. Another time he received a cut eye which duly swelled. The blood was pouring from him but he was all for having another go at that "joke of a hill". All we could do was to drag him off home.

Though hopeless at slaloming, he took a trip to Bakuriani. He later laughed ruefully: "Just look at me. I must be totally lopsided the way I've been falling on one side."

Nature did not endow him with an exceptional physique. He was physically not very strong, nor was he particularly dexterous. Tamm needed willpower, perseverance and courage to accomplish things others did with ease. He did not know how to and did not want to pace himself. He always stretched himself to the limit and at times overtaxed himself. His mountaineering, I feel, was a case in point.

And the way Tamm worked! I did the calculations for him when he was busy developing his isobar theory. He always worked at home, so I often had to call on him. The air in his study was usually thick with cigarette smoke — he had had no time to air it. Natalia would complain to me that Tamm had again worked until dawn and was smoking like a chimney. He would rise at 10 a. m. after very little sleep and get back

to work, and that was his routine day in and day out. She was helpless to do anything about it.

His huge writing desk was buried under papers all covered in his sharp hand. But the papers looked pell-mell only at first glance. Actually they were all sorted over the length and breadth of the desk top in patience-like patterns. Tamm never had to look for a sheet; he always knew where he had put it.

Those were pre-computer times, and the amount of calculations I had to do on a "Mercedes" machine was enormous. Four parameters had to be fitted, and a formula on seven full pages had to be used for each point. And those points had to be plotted on the experimental pion resonance scattering curve. It often would appear that some point had been missed out and when I was about to start working at it, Tamm would beg me: "Maybe we can shift it just a bit?"

He never went about advising anyone. Respecting the independence of his staff members, he just requested them to do something. For me even such requests were unnecessary: I became fanatically devoted to the project myself. The isobar theory came into being at long last, was printed, and was reported on at an international symposium.

In the last years of his life Tamm was looking for a new consistent quantum field theory. He called it a "lottery with a negligible chance of winning" and worked at it till his last days, covering mountains of paper with calculations, but without success.

I have mentioned his youthful fervour and enthusiasm. But there was nothing childish in his conduct. His manners were impeccable and it was only to be expected that Tamm would be such a great success with the King of Sweden when receiving his Nobel Prize.

Physicists of my age called him Igor. But never in his presence: one felt that the hail-fellow-well-met style was not to his liking. Nor can I recall any of his colleagues with whom he was on first-name terms. One felt that familiarity was against the grain with Tamm. And loutishness enraged him. I witnessed Tamm making short shrift of a young chap who, sprawled in his armchair, gave Tamm some paper to sign. Tamm stopped in front of the young man and in a quiet (oh how tranquil!) voice told him: "Get to your feet when you speak to an elder man!" The fellow leapt up as if shot from a cannon, Tamm passed by and left the room. The scene was magnificent.

That happened a long time ago when our Department was still in the old building in Miussy. Later on it moved to new premises and took up several rooms on the upper floor over

the library in the new building. The large group of young physicists who joined it at that time were to form the backbone of our Department. Other departments and laboratories were taking on new staff all the time, while our unit remained practically unchanged. I think that was the way Tamm wanted it. But the list of postgraduates and research workers temporarily attached to the Department, kept growing and a crowd of them filled our corridors and stairways.

The image of a "good" chief or manager is today taken to mean him knowing all his subordinates well and their concerns, helping them, giving them advice and generally caring for them.

Tamm was very far from such a mould. Administrative matters seemed absolutely alien to him. Naturally enough, he did discuss the key issues in the life of the Department with those close to him among its leading figures. He was hardly aware of the progress in the work of each of his subordinates. All members of the Department were absolutely independent research workers, cherishing the opportunity to do their research their own way. If anyone asked Tamm for advice, he most certainly received it. But I noticed that our staff members rarely approached him: they felt that his time, his own research work were too precious. But no sooner would some new scientific development come someone's way, no matter from what branch of science, than he would race off to Tamm with the news, knowing full well how welcome such tidings were.

So he was not the "ideal leader", but his probity, straightforwardness, refusal to compromise alongside his conscientiousness and truly heroic capacity for work at the end of his life made him the symbol of honour and dignity for our Theoretical Physics Department, indeed its very heart and soul. We who knew him for so many years will never forget that. And we are anxious to pass those feelings on to younger colleagues who did not work with Igor Tamm.

S. M. Raisky

AN UNOFFICIAL RECORD

The Tamms and the Mandelstams. In the late 1920s and early 1930s one could enter the Mandelstams' flat on the ground floor of the old building at Moscow University's Physics De-

partment through the front door overlooking the garden that separated it and the Chemistry Department. But another door, usually open, connected the apartment with the Institute of Physics. A short, narrow passage led to two rooms in the Optics Laboratory and into the main corridor of the ground floor of the Physics Faculty. The corridor was flanked by laboratories.

Igor Tamm, Grigori Landsberg, Frida Landsberg, Mikhail Leontovich and Nikolai Papalexi (when in Moscow) would often call on the Mandelstams on business during the day and after hours for evening tea. Younger men and women also attended those tea parties. The nephew and nieces of the Mandelstams were permanent fixtures, and so was I as their relative. Those were regular high teas, minus the hot course, with bread, butter, cheese and sometimes sausage. By that time Leonid Mandelstam would usually join us. The chat round the table covered a wide range of subjects: family news, recent developments in the Chair and laboratories, at the University and in the Academy of Sciences, and home and international affairs. Jokes were told with gusto. Those who gathered around that table had no secrets from each other, and everything they thought about life and their professional concerns was quite openly aired. One topic replaced another and only our uncompromising attitudes remained unchanged.[1]

Leonid Mandelstam did not hold any executive post at the time, neither did he head any chair or laboratory; he just took a seminar (each year on a new theme) and a colloquium. Those seminars had a profound influence on the curricula and syllabuses of a number of courses in the Physics Faculty at Moscow University, on various research papers and textbooks and on the research projects of the Institute of Physics.

Mandelstam worked at home leaving it only rarely for the Lebedev Institute or the Presidium of the Academy of Sciences. Current and forthcoming projects were discussed with young scientists and postgraduates at his home. These included M. A. Leontovich, A. A. Andronov, S. E. Khaikin, G. S. Gorelik, S. M. Rytov, S. P. Strelkov and, somewhat later, V. V. Vladimirsky.

A complete overhaul of research and the teaching of physics was badly needed at that time to bring them into line with the new physics, the theory of relativity and quantum mechanics. Igor Tamm led several theoretical physics researches at the

[1] See also S. M. Raisky's reminiscences in the book *Academician L. I. Mandelstam: Birth Centenary*. M., Nauka Publishers, 1979.

University and, subsequently, at the Lebedev Institute. He was involved in solving some of the vexed problems brought forth by such reconstruction. Training a new generation of research workers and lecturers capable of dealing with modern scientific ideas presented the greatest difficulty. Besides, the opposition of influential conservatives had to be overcome and ignoramuses and demagogues brought to heel.

Mandelstam, Papalexi, Landsberg and Tamm were all men of the older generation linked by a common background, colleagues in joint research projects, and initiators of important research programmes and of new physics courses for secondary and higher schools. They were a group in regular contact with each other and not an isolated group. In Moscow much credit for such an overhaul is also due to other outstanding physicists, especially Professor S. I. Vavilov of the Physics Faculty at Moscow State University.[2] But that ought to be told in greater detail.

Tamm's line of research was much further from Mandelstam's than that of any other member of the group. And still, as far as I remember, these two were always debating fundamental problems in physics and comparing notes on new ideas by prominent scientists, not to mention current affairs in the Institute.

* * *

Landsberg is enraged. In the 1920s and 1930s scientists would bring home books and laboratory supplies from visits abroad. Once G. Landsberg bought in Germany a pack of "Ilford-Monarch" photo plates, millimetre paper, a jar of vacuum cement and a bar of picine. His prize purchase, however, was a quartz tube approximately 60 cm long with 2 mm thick walls and 3 cm in inner diametre. The precious appliance he planned as a component in a "Wood's horn" for light scattering studies.

Now imagine a laboratory: a case full of instruments, a writing desk, three chairs, a mercury vacuum pump humming away and two lab benches, one with the experimental unit, the other covered with all sorts of lab paraphernalia. The priceless tube is lying there close to the edge of the bench. Landsberg

[2] Work together brought forth a lasting friendship between Vavilov and the scientists listed above. Vavilov played an important role in getting Mandelstam invited to Moscow University (see: *Sergei Ivanovich Vavilov: Portraits and Reminiscences*). M., Nauka Publishers, 1979, pp. 153, 155, and others) — *Ed.*

in a white smock is sitting at the desk drawing a "Wood's horn" layout. The door opens and in comes Tamm. At the doorway, he is already talking to Landsberg. Passing between the cupboard on his right and the lab bench on his left, he casually takes up the shining quartz tube, and waving it carelessly advances towards the desk. Landsberg, who, having seen Tamm, gets up with a welcoming smile and stiffens in horror at the quartz tube pirouetting in peril.

Landsberg evidently knew how to get a child off the sill of a high window: go up to the child, stop at an arm's length away, talk to it soothingly and quietly all the time. And that is exactly what he did. Keeping up the conversation Landsberg at the right moment got hold of the tube close to the place where Tamm was holding it, loosened Tamm's grip on it, turned about, opened the cupboard, put the tube inside, locked the door, put the key in his pocket and sank into his chair exhausted. The whole scene was over without the "star performer" even noticing that something was wrong. I was the only one to enjoy the pantomime.

* * *

Igor Evgenievich climbs a tree. Not being able to do something? Pulling up short of an obstacle? These were situations Tamm thought he would never find himself in. Watch others skiing down a steep slope without having a go yourself? No, that was just out of the question. He had to follow suit immediately and recklessly at that. And what if the run ended in injuries and broken bones? Well, to pot with the danger, you couldn't expect him to stand there watching on. Once we were walking in a forest. I asked Tamm which of us could climb a tree that we saw faster. Without a word, he jumped up to the lowest bough and began to climb for all he was worth.

Once in summer our pal Dima (now known as Vladimir I. Arnold) raced past on his bicycle. Tamm was eager to display the same dexterity and asked Dima for a go. He got off to a poor start, catching his trouser leg up in the chain, but then he rose to the occasion and was delighted with his performance.

* * *

The Tamms and Leningraders in 1942. During the War a large group of Leningrad scientists and their families were in evacuation in Kazan. From the autumn of 1941 on, Kazan was host to many Moscow Academy of Sciences Institutes. The newcomers

were put in the huge gymnasium at Kazan University and various catering and medical facilities of the Academy were assigned to them. Those who came from blockaded Leningrad had suffered great hardships, lost many dear to them, and the University and Academy staff did their best to help them. The Tamms did it their own way. They invited a Leningrad woman with pleurisy who was carrying a sick baby in her arms to stay with them and shared what they had. The Tamms were a large family (a grandfather and two teenagers — a son and a daughter — lived with them) and they did not have it easy, but everybody worked hard and had other things on their minds.

It is appropriate to mention that Tamm's son, still a schoolboy then, worked as a lorry-driver, while their daughter compiled descriptions and Operating Instructions for various spectrometric instruments manufactured as part of the defence effort by the Optics Shop. Tamm also lent a helping hand on that project — he did the calculations for the condenser system of the spectroscopic unit.

* * *

Ashamed. When Tamm was elected a Full Member of the USSR Academy of Sciences (and many thought it was long overdue) he requested N. A. Raiskaya to suggest somebody he could help: "I have received a lot of money, and I shan't feel too good about spending it on my own needs. I would like to help somebody... Giving someone a chance to study is what I would like best." So a girl who lived in a basement with her janitor mother and a blind younger sister was offered an allowance to continue her studies and get through college which she did. Either Tamm or his wife brought the money to Raiskaya once a month (only on a few occasions did she call on them to collect it). Tamm did not know who the girl was, nor did she know the name of those who helped her.

Not all of Tamm's views on abstract subjects could be accepted without reservation, but his reaction to specific events and human conduct was instantaneous and correct. It seemed to take no mental effort to be built-in. Without fear or procrastination Tamm was always in the thick of the battle for a sound and honest state in Soviet physics. Many modern physics achievements are the extension of Tamm's stance. Tamm's memory will live.

V. I. Ritus

TWENTY YEAR ASSOCIATES

In 1951 I was assigned to a team of theoretical physicists led by Tamm. The group was doing research on applied problems. The assignment was a complete surprise to me. I had just graduated from the Experimental Division of Moscow University Physics Faculty and had enrolled for a postgraduate course. I was certainly interested in theoretical nuclear physics, and the big names such as Landau, Tamm and Fock were familiar to me. For reasons I could not have explained Igor Tamm was the man I wanted to work under. But believing that I lacked sufficient knowledge to take up theoretical physics, I was intending to become an experimenter. However, events unfolded themselves in total disregard of my doubts. Such Soviet physicists as Zeldovich, Ivanenko, Landau, Leipunsky, Lukirsky, Petrzhack, Tamm, Flerov and Khariton were known to university freshmen through books on the nucleus and for me Tamm's name was always associated with nuclear force problems which made it very attractive.

At a later stage I came to know other lines of research Tamm was engaged in, especially the Vavilov-Cherenkov effect in electrodynamics and solid state theory. And yet, the greater part of his life Tamm devoted to nuclear force theory and the theory of strong interactions of elementary particles.

In 1953 the remarkable nucleon-pion interaction experiments of Fermi became known. That pions account for proton-neutron nuclear forces had been established long before. In 1935 the Japanese physicist Yukawa had, in effect, predicted the existence of those particles considering a short range of nuclear forces. Yukawa's prediction was based on Tamm's work in which it had been supposed that internucleon forces are the result of light particle exchanges, a phenomenon similar to the photon-exchange origin of electromagnetic forces acting between two electrons. In 1947 pions were discovered experimentally. Six years later Fermi used pions, generated in a proton accelerator, to bombard protons and neutrons. The experiments yielded direct and detailed information on pion-nucleon interaction. The interaction, it appeared, was of a resonance type, i. e. if the pions possessed some definite energy, their scattering by nucleons was especially strong. In addition, it was heavily de-

pendent upon the pion and nucleon charges. The experimental data thus obtained urgently called for a theoretical explanation.

Tamm advanced the idea that in the process of a pion-nucleon interaction the nucleon can go over to an excited (isobaric) state, that is become a particle similar to a nucleon, but an essentially heavier one and with a large variety of properties: such an isobar has a greater proper angular moment and four charged states (as distinct from the nucleon with its two states corresponding either to the proton or to the neutron). Tamm with V. L. Ginzburg had already investigated the properties of high-spin particles. Now the order of the day was to study the interaction of those particles with pions and to find out how such interaction could be used to describe the scattering of the pions by nucleons.

Tamm and his staff brought this extensive project to a successful conclusion. However, the work did not stop at that. Investigation of the effect of the new isobaric state upon the electromagnetic interactions of the nucleon was called for as the next stage. Such an effect was expected in the photon-nucleon interaction both when it leads to the production of pions, and when the photon-nucleon scattering is elastic. Tamm proposed I set to work on a theory explaining the photoproduction of pions by nucleons.

I anticipated that Tamm would follow my calculations, correct my errors and offer guidance. In fact Tamm merely wrote out at the outself the free isobar motion function, and the operator he had found a long time before (sometimes referred to as the Tamm-Casimir operator). The rest of the "charge" came from the highly intensive research Tamm was doing himself and his accounts of the results he and his staff had obtained. In about a year the project was completed providing quite an adequate interpretation of the experimental data that had been produced by that time.

Tamm's isobaric idea was thus confirmed.

When I went over fully to elementary particle physics and attended Tamm's theoretical seminar regularly I realised that his prime interest lay in the physical principles of the matter under debate. He showed no particular interest in the details of the calculations (though he was a master at the mathematical side). Where he came into his own was showing the physical essence of phenomena studied and expounded by others. The actual need of a piece of research and its being essential in principle were of key importance to him: hence his nuclear force studies, his controlled thermonuclear fusion project and his quest for an internally consistent quantum theory. The hypotheses

I. E. Tamm at a seminar (1960)

advanced by Tamm were always specific and could be confirmed or rejected by crosschecks against experimental data or through logical analysis.

When one of his senior students set afloat several quite abstract ideas on cosmology, Tamm was annoyed. He told me such hypothesis could be neither confirmed nor refuted for the foreseeable future. This was Tamm's passionate temperament coming out. He found as much satisfaction in the creative process as in the results. Some physicists, however, are content with beauty and harmony in their ideas and do not care whether others understand them or whether specific results confirm them.

In 1953 our main applied research project was completed, with the installation we had worked on successfully tested (I was on holiday at the time). Tamm was genuinely glad that things had been seen through. We chanced to meet at Gagry. In a remote corner of the passenger hall of the local port, he reproduced the strain and excitement of the final stage and all the complications involved. Tamm had seen many of those he had known before in a new light. He was struck by the exactness with which an experienced experimenter visually determined the efficiency of the installation. He stressed, in particular, the organisational ability of the late V. Y. Gavrilov.[1] Subsequently Tamm assisted Gavrilov in setting up the molecular biology laboratory at the Kurchatov Institute in Moscow.

Another feature of Tamm's make-up was his intolerance of pseudo-science. He reacted sharply to such manifestations even in the more innocuous cases. Once one of the young staff members of his Department noted that a new shaving mirror with a slightly concave surface produced a magnified image, as distinct from its old flat-surface variety. On hearing this, his colleagues living in the same residence resolved to play a practical joke. They unanimously declared the reflections in both mirrors to be absolutely identical so that their friend brought the two mirrors to the Department and began a survey of everyone's judgement as to the size of the facial images. Some (whom the jokers alerted) said the reflections in both glasses were identical, while the rest said one of them gave a magnified reflection whereupon the surveyer came up with the curious hypothesis that just as some people were colour-blind many could not distinguish a normal-size reflection from a magnified one. Tamm refused point blank to have anything to do with the joke. His reaction was quite unexpected to us: "No show-off-hand solutions for science," he said, with even a touch of bitterness.

Tamm's judgements, reactions and overall conduct were absolutely unaffected, natural and straightforward. Sometimes he could fly into a temper and reprimand someone sharply (but never harshly). Certain future Academicians were among those who came in for his wrath. Soon he would cool off and, having muttered an excuse, take up with the culprit some business in hand. Nobody ever heard him use cross or unprintable language. The greatest liberty he allowed himself once in a while was the expression: "A muddle like staying in a self-made

[1] Tamm's praise of V. Y. Gavrilov's activities has also been noted by V. Y. Frenkel and B. M. Bolotovsky. See their articles in the present collection — *Ed.*

puddle" (he usually used it when international developments were discussed). It would be in place to note that Tamm was more perplexed by the acuteness of interethnic conflicts than those between states...

When asked to lend a helping hand in some public or private matter Tamm would always inquire "What should I do?" and would act without delay as requested. I once had to ask for his assistance. A few years after that when I went to Moscow City Hall on a private matter an official told me: "I remember your name. Academician Tamm once put in a word for you." And he wrote on my application: "Request should be granted." I had fortune on my side from that day on, and the now bedridden Tamm was very glad.

I knew Tamm for 20 years. His nature changed somewhat over that period in that he grew softer but he was definitely to the end the most energetic and cheerful man I knew.

In 1967 I was in a group from our Department, to be sent to a conference at Heidelberg. Tamm recalled to us staying in Germany in his youth. Heidelberg he associated with a verse that he recited to us in German. He suggested I write it down and use it on some suitable occasion. Writing it down I got mixed up between German and English spelling. Tamm noticed this, and wrote the verse for me. Unfortunately, I do not know who the poet was (neither did Tamm) but the gist of it was: the merry students of old Heidelberg not only unveil the mysteries of science, but drink wine and look into the waters of the Nekkar to see blue eyes of girls. I was struck by Tamm's memory and his vitality. With great emotion he told us of Germany, his contacts with physicists who were young then and became world famous scientists later, and spoke of the amiable university life of the day.

Tamm was a courageous man. I was impressed by the following thing I learnt from him when we met at Gagry, though he himself thought nothing of it. It appeared that he had come to Gagry crossing the Caucasian Range alone on foot, sleeping out in the woods in his sleeping bag. Fortitude and an indomitable spirit were with him till the last.

On one of my visits to Tamm when illness had already chained him to the iron lung, his wife Natalia asked me to mend the light in her kitchen. Fuse had blown, and the fuses were of a design unknown to me. I began to tinker with them while Natalia Tamm and the nurse gave a running commentary. I knew that the iron lung was connected to the lift's power supply which never went off even if the whole building was blacked out so I felt sure of myself. Suddenly we heard Tamm shout —

the iron lung was off! Several dreadful seconds passed before it dawned upon me that I should press the button protruding from the plastic fuse case. The iron lung started up again. I went over to Tamm and he stared at me with silent reproval. Before long the situation was back to normal, and we could carry on with our chat.

* * *

I have presented here a ragbag of reminiscences which can form no more than a few strokes to his portrait. Among many remarkable works Tamm's passion for science and creativity stands out. He was never snobbish, haughty or donnish and fostered innovation at every level. Such a climate was conducive to the formation of scientists and, without the least doubt, has borne good fruit.

S. M. Rytov

THE OLD DAYS

We were in our fourth year at Moscow University's Physics Faculty when Igor Tamm read us a course of lectures on field theory in 1928. About the same time I began to attend the seminars sponsored by L. Mandelstam where Tamm was a regular fixture. In electronic engineering L. Mandelstam collaborated mainly with N. Papalexi and S. Khaikin, in oscillation theory with A. Andronov and A. Witt, in optics with G. Landsberg; his immediate associates in theoretical physics were I. Tamm (quantum mechanics, then known as wave mechanics, and the theory of relativity) and M. Leontovich (statistical physics). The scientific interests of M. Leontovich and I. Tamm were certainly not limited to these areas; nor does it represent a watershed between them. Both were involved in a broad range of research in theoretical physics under Mandelstam, although each already had a defined area of preference.

Tamm was obsessed with theoretical physics, and this everyone sensed each time he gave a lecture or addressed a seminar. As far as I know, and I have nothing but hearsay to rely on, he once took a general physics course and rumour attributes a few curious incidents to the attending demonstrations.

Once Tamm was demonstrating the Zhukovsky bench, a small round platform that rotates freely on a vertical axis. He took a hefty briefcase, such as students use, in each hand, placed one foot on the platform and, spreading his arms he pushed off with his other foot. The thrust must have matched his flamboyant temperament, so that when he brought the briefcases to his sides, the platform began rotating at runaway speed. Once or twice he was able to spread his arms again, but as he tired, things reached critical point.

"Stop me", he cried, but either the students misinterpreted the situation or they were hesitant to grab a professor's hand, as Tamm's arms slid downwards, the platform gained speed. His command was loud and clear: "You in the front row, get up here and stop me!" The order had the desired effect, and all ended well.

I would not suggest that the episode explains the fact that Tamm never again gave a general physics course. I rather suspect his ambition was to expound theory on a higher level than a general course of physics allow. For Tamm a higher level did not necessarily entail mathematical sophistication. He could have made himself an excellent populariser if that had been what he wanted to do.

The Znaniye ("Knowledge") Society did not exist then, and many institutions of higher learning, of which Moscow University was not the least, took it upon themselves to spread learning in physics, the students making the arrangements. I arranged for Tamm to meet workers at a Moscow electrical engineering factory and talk on electromagnetic field, a subject of his own choosing. I accompanied him and was greatly impressed by the way he treated the counterposition of "short-range" (i. e. point-to-point) and "long-range" (action-on-distance) ideas of electromagnetic interaction, the centrepiece of his lecture.

Some philosophers of the day tended to vulgar sociologism. They put "short-range" and "long-range" interaction in terms of a contrast between materialism and idealism. To be more exact, they maintained that the attitude to the problem was a criterion for distinguishing between materialists and idealists. As he substantiated a case for short-range interaction, Tamm took pains to guard his audience against identifying short-range interaction with vulgar mechanistic concepts. Questions and remarks from the floor showed that he had got the subject-matter and his ideas over to the audience.

Tamm was particularly enthusiastic about lecturing on field theory, which is not surprising considering that he was

working on the final chapters of what is now a standard textbook on the theory of electricity. We knew nothing then about his work.

As soon as he had finished that textbook, his lectures switched to new areas of interest. A series of lectures in the spring term of 1929, "The Physical Foundations of the Theory of Relativity", dealt mainly with the general theory of relativity. It was essential learning for a subsequent course in April 1929, "Einstein's Theory of Gravitation and of Electromagnetism". Here Tamm lectured on a new work by Einstein, one of Einstein's first attempts to evolve a unified theory for gravitational and electromagnetic fields.

Tamm showed a keen interest in the theory. He went beyond simply introducing it to us to endeavours to perfect it. He felt that, with the help of quantum mechanics and the Dirac equation, the difficulties besetting it could be overcome. Five of Tamm's works on these subjects were published in 1929 (two of them jointly with Leontovich). We students, were not aware of these works, nor would our knowledge have been sufficient to make them out. Yet in his lectures he presented them in a quite understandable manner. This union of his current research with his lectures was a feature of Tamm the lecturer.

A group of Tamm's own students (S. Shubin, D. Blokhintsev, S. Altschuler and others) took shape in the 1930s to become his own scientific sector studying the photoelectric effect and electron behaviour on the surface of crystals along with a number of other areas in quantum mechanics. Then came his involvement in elementary-particle physics and nuclear-force physics. All of this was a far cry from the theory of non-linear oscillations, my research subject under Mandelstam and Andronov. When late in 1934 the Academy of Sciences moved to Moscow where the Lebedev Institute was subsequently established, I began to see more of Tamm again. Tamm took charge of the Institute's Theoretical Department, while I took up a job in the Oscillations Laboratory.

The seminars and conferences sponsored by the Institute took up all of its small staff. Regrettably, my memories of them are rather vague because I was engrossed in theoretical and experimental work, at first in Landsberg's optical laboratory and later in the oscillations laboratory under Mandelstam and Papalexi. Needless to say, all of the staff and Mandelstam's students continued to regularly attend his seminars and lectures at Moscow University.

A seminar arranged by Mandelstam in the 1938—1939 academic year dealt with a number of problems of physics, with the Vavilov-Cherenkov effect first in the running order. Tamm re-

ported on the theory of the effect. I have already mentioned Tamm's total "engagement" in whatever point of physics he happened to be discussing, with which he always infected his audience. But on that occasion he outdid even himself.

And little wonder, since he and Frank had only recently produced a theory for the effect which Mandelstam described in his opening remarks as exquisite. Tamm talked about a superlight electron in a medium and about the acoustic radiation of bodies travelling at supersonic speed, about similar waves on the surface of a liquid, and the like.

His fervour and excitement made him speak ever faster than usual. It was evident that the speed with which Tamm spoke was a direct outcome of his way of thinking. His words could not keep pace with his racing thoughts.

The Mandelstam school was scrupulously accurate in quoting from the works of others. Tamm honoured the tradition, so much so in fact that he did more than what was strictly prescribed by it. Here is one case in point. It relates to a problem I formulated, but never finalised, a transition to very short wave lengths termed in optics as geometro-optical approximation. My intention in working on a doctorate was to apply the transition not merely to Maxwell's equations, but to Dirac's equations which led to four definite conditions. One expressed the conservation of the number of particles (a fact mentioned in a work by Pauli published earlier) while the other three were supposed to describe "polarisation", i. e. the change of direction of the electronic spin when an electron follows a classical trajectory. But as distinct from the case of electromagnetic waves, on that occasion I failed to find simple and lucid expression for those three conditions.

When I asked Tamm for advice, he said this problem as applied to quantum mechanics was less intriguing than a similar problem for electromagnetic waves, given the fact that there the polarisation could be observed directly, whereas a change in the polarisation of electronic waves could only be judged when spin affects the particle's trajectory. Tamm's argument was so convincing that I abandoned further efforts, recording the view at the end of my doctoral thesis.

However, two years later Tamm modified his view. He asked one of his postgrads, A. Galanin, to study the problem with application to mesons as well as electrons (Tamm was very much into mesons at the time). Galanin brought the problem to conclusion in his one year as a postgraduate student before the War in which he served in the army.

Nothing appeared to stand in the way of the publication of two articles written by Galanin. Yet Tamm made a point of seeing me at my laboratory to ask me whether I had any objections to the publication given that the work would mention the origin of the problem. Tamm felt that those who formulated a problem as well as those achieving the ultimate result must always be credited. Regrettably, commitment to this sort of tradition today is often slack.

The popular notion of the theoretical scientist as someone who confines himself to talking or writing on the subjects of his interests and pursuits is, of course, a false stereotype. There are any number of examples refuting such ideas, and Tamm is perhaps one of the most striking. His varied activities always showed the man of action. If he was intrigued by the "Incredible Snowman" or a cave (where legend had it there was hidden treasure), he did not only read the literature and engage in idle talk, but insisted on joining an expedition to see it all with his own eyes.

I suspect that the "sporting challenge" was just as much part of his quest for answers in theoretical physics when he sat up at night, doing mathematical calculations. He had to be in the very thick of events to assert the scientific truth and help its martyrs.

I remember Igor Tamm as a man full of life, energy, vigour and enthusiasm.

V. P. Sassorov

AN ALPINE ETUDE

The job of the expedition to the high mountains of the Central Tien Shan in 1932—1934 was to make a general geological survey of an area inaccessible even to geologists. For some of Tien Shan's ranges and streams only hypothetical maps existed.

I was in charge of the expedition's mountain climbers whose job was to make an eye survey, explore the "white spots" or areas that could not be reached by geologists, and ensure the expedition's passages across difficult terrain.

In the early spring of 1933 I had received an invitation

to see Professor Tamm. I knew nothing about him, except that he was the author of a textbook I had studied as a student at the Leningrad Polytechnic. Needless to say, it was a feeling of trepidation that I contemplated my first encounter with this distinguished professor. I had no idea of the purpose of the meeting and I felt quite considerable anxiety. However, Tamm turned out to be so homely and affable that he overcame straight off my shyness and fears. At 38 he struck me as a very outgoing person and what you might call a jolly good fellow.

Tamm asked me for permission to join the expedition, saying he had an overpowering desire to be among pioneering explorers. In those days taking people on involved a minimum of formality, and he was immediately signed up as "draught power", a unit unknown in classical mechanics. It works out roughly at 55—65 lbs lifted in 10 hours a mile and a quarter up over fairly difficult ground. This process repeated itself many days in a row.

Tamm's academic prowess was not piqued by his new role as a mule. To him the most important thing was that he was going to mountains hitherto never seen by man.

Starting out from Frunze, the expedition was to cross Lake Issyk-Kul before converging on the town of Przhevalsk (Karakal), the point of assembly. Young people bursting with energy when reduced to inactivity, such as aboard a motor ship, are bound to crack jokes and engage in a considerable amount of leg-pulling. Our group included a well-known mathematician Dr. B. Delone, the pioneer of Soviet mountaineering, an indefatigable story-teller, a man of ideas with a gift for repartee. To cut a long tale short, we were egged on to dive from the upper deck, and needless to say, Tamm was among the divers. He was flamboyant, temperamental and as sociable as anyone. Besides, the group's reputation was at stake.

We reached the Inylchek Glacier after many days on the march and pitched camp at the foot of Nansen Peak.

The first major trek was to take us across the Inylchek Range east of Nansen Peak. It was a demanding route, technically and physically, one largely for the mountain climber. Our job was to find out where it led by making observations as we moved on.

The walkie-talkie was not in existence at the time, nor did we need one for there was nobody to communicate with. The three of us were on our own. Seventy-five to ninety pounds was the standard pack. When we gave Tamm only 65 pounds, he indignantly demanded an equal share. Arguments that we were younger, more experienced and so on were to no avail. He would have no allowances.

For the first few hours we trailed across the moraine of the Inylchek Glacier, before turning right to the glacier's left tributary. Now we were heading into an area where none had set foot before. As pioneering explorers we felt that we were justified in naming the glacier "Trail Blazer".

Trudging along a cracked moraine is in itself exhausting. Heavy rucksacks certainly did not add to our agility, and it was excruciatingly difficult. Seeing our stooped figures and drooping arms an onlooker might have imagined we were trying to pick something up from the ground. All we were in fact trying to do was to counteract the downward pull of the heavy rucksacks. When you can see nothing but the points of your shoes, and that only through trickling sweat, you are not up to enjoying the beauty of the landscape.

After 12 hours on the move we settled for the night at the foot of an icefall. The Inylchek Glacier was now far below, and above we could see the ridge we were to cross. The 1,5 mile-high wall of snow firn was a true laboratory for avalanches which reached us in the form of snow powder to a rumbling accompaniment. Plodding up such a slope is fraught with dangers. The ascent should only be undertaken after a thorough reconnaissance coupled with special precautions... But in those distant days when Soviet mountaineering was in its infancy, we had different notions of what was possible and impossible, permissible and impermissible. Consequently, we performed feats, like those inventors of whom Einstein said they had had accomplished the impossible because they did not know it was impossible to achieve. It was not recklessness, it was just the way we were.

At dusk after dinner, tea and supper all together we scrambled into a carefully pitched silk tent, a gift to Tamm from Dirac, and listened to the mountains. Even Tamm was silent, a sure sign that he had been lulled by the rucksack and the moraines.

At dawn the weather changed sharply for the worse. Heavy wind and snow engulfed the tent. As they say, mountaineering is not the best way of wintering a summer. Tamm and I spent the next day locked in battle in a board game to the booming accompaniment of avalanches. Tamm became fidgety as he lost one round after another. He changed his tactics, made calculations, said something on the subject of probability, expounded game theory, made every attempt to show that his reasoning was logical and well-grounded and yet continued to lose. He could not accept this and was visibly upset. But when I suggested that alpine disease might be to blame, that settled the matter. Later, when we returned to base camp, cheerful Tamm lounging

on grass among trees suggested another game just to test the effects of alpine disease.

At this point I had to confess that I had, in fact, been pulling his leg. He jerked, then began advancing towards me with a clear determination to narrow the distance between us. It was only the swift reaction of a young man that prevented long-range interaction precipitating into short-range interaction, and Tamm's emotional outburst came to nothing. My frank confession had mitigated punishment, while Tamm had regained confidence in his mental powers. He did not accept trickery between friends.

Anyway, towards evening the weather began to clear. The next day we were to cross the icefall and get to the ridge if we could make the 1,5 mile climb. If we had followed the rules prescribed for modern mountain climbers, we would have waited a couple of days for the avalanches to come down. Fortunately, they had already done so when we crossed the icefall and approached the starting point for an onslaught on the ridge. We thought the chance of another avalanche on the same day very remote.

We took a lot of safety precautions so it took us a whole day to get to the ridge. We reached the highest point at 17,250 feet towards evening. If we had taken a turn to the right and moved along the ridge, we could have got to the summit of Nansen Peak without much difficulty, but that was not in our plans.

With night approaching, we stayed on the ridge for a short time only. After sketching the area and taking pictures and measurements, we started down southward, without quite knowing where we were heading. Our hope was that we would ultimately find ourselves on the Kaindy Glacier. We spent the night on the snow in the cirque of an unfamiliar glacier.

Next day, after a descent of several hours we stumbled into an open glacier only to realise that for several days we had been going in circles around Nansen Peak which had all along been to the right, although we had shifted direction from east to west. We were on the At-Jailau Glacier, not Kaindy. In a couple of days our group reached base camp having moved round Nansen Peak. Seen from At-Jailau Glacier in the south, the peak did not look as forbidding as it had seemed when we saw it from Inylchek Valley in the north. Later, on the strength of our reconnaissance, a group of mountaineers made a pioneering ascent of Nansen Peak.

I have told you about just one of our many expeditions. Some lasted 15 days. During others from dawn to dusk we would scramble over rocks and make half-mile detours to skirt an obstacle to gain a mere 100 metres of horizontal advance. For

days on end we had to do without water. There was no end to what we went through. The romanticism of mountaineering has a price: dripping sweat amidst severe cold, the salt on your back, sleeping under a blanket of snow, losing equipment, horses and indeed people in crossing mountain streams, the hunger for a hot meal, the privations of a nomadic life and much else. An encounter with a snow-leopard or a bear may be romantic, but it is more than just that.

Tamm was perfectly cut out for the roving life. He was tough, both physically and morally, a reliable person and an excellent storyteller in the hours and days of forced inactivity. He was a man with whom you would go on an expedition to either of the poles. Such was the Tamm I knew. I can't talk for him as the scientist. Perhaps not all the collocations used here are conventional but then almost all people are young once and some remain young into their old age. Tamm was such a man.

N. V. Timofeyev-Ressovsky

A DIALOGUE BETWEEN BIOLOGISTS AND PHYSICISTS

I am a zoologist, biogeocenologist, geneticist and evolutionist rather than a physicist. Yet way back in the 1920s as a pupil of N. Koltsov and S. Chetverikov I became involved in the study of phenogenetics, population genetics and the mutation process at the Koltsov Institute of Experimental Biology. At a Berlin research institute I started experiments in radiation genetics, the impact of ionising radiation on mutation. This science was just beginning to take shape following H. Möller's pioneering research. In connection with the ideas of the physico-chemical nature of chromosomes and genes as advocated by N. Koltsov from the 1920s, I decided to find out whether processes whereby ionising radiation affected mutation could be seen in a biophysical context in radiation-genetic experiments. In this way we hoped to understand some aspects of the physical-chemical nature of genes. Geneticists, cytologists, biochemists and physicists from Berlin research institutions regularly converged on my laboratory at Buch near Berlin to discuss new problems of the theory.

In the 1930s some of the eminent theoretical physicits working under Niels Bohr in Copenhagen got into biological matters, notably the nature of genes and mutation processes. At the time I was doing research with Max Delbrück, a young physicist with close links to Bohr's circle. Through him my associates and I established contact with the Bohr group of physicists and Bohr himself. Right up to the War some 20 scientists from my circle in Buch and Bohr's Copenhagen colloquium used to join with a group of Parisian physicists, chemical scientists and biologists twice a year for a week to discuss general matters in theoretical physics, biochemistry, cytology and genetics, which were of interest to all of us. We met in small seaside towns in Denmark, Holland and Belgium. The Buch circle, participation in Bohr's colloquium and especially in those international gatherings which with common methodological interests as a base sought the roots of important life phenomena, prepared me, the zoologist, for communication with theoretical physicists.

My contact with them and the mathematicians grew in the late 1940s when I worked at a number of laboratories, and from 1955 on when I joined the Urals branch of the Academy of Sciences. It was at that time that I established close contact with the Vonsovsky group of theoretical physicists at Sverdlovsk, the cybernetic mathematicians under A. Lyapunov and the large group of young scientists L. Blumenfeld had gathered in the Biophysics Chair of the Moscow University Physics Faculty. They all met regularly for a couple of months at our experimental biology station in Miasovo in the South Urals. They came from Sverdlovsk, Moscow, Chelyabinsk, Novosibirsk, Leningrad, Irkutsk and other cities. There the presentation of scientific papers and the ensuing heated debate produced common methodological platforms for a fairly broad cross-section of young scientists. These platforms were to stimulate modern biological science in this country.

In the late 1950s steps were taken to extricate biology, and particularly genetics, from the dominance of dogmatic concepts.[1]

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[1] "However, the development of biology in the USSR was not marked only by periods of advances and discoveries. In 1936 and 1939 there were a number of sharp debates on methodological problems of theoretical biology. During these debates some tenets of genetics and Darwinism and the principles of selection based on them were exposed to sharp, subjectivistic criticism. A group of scientists (T. D. Lysenko and others) defended erroneous, mechanistic views of the nature of heredity, speciation, natural selection, organic expediency, and so on. These views were proclaimed to be the development of the scientific legacy of the prominent Soviet breeder I. V. Michurin, and they were labeled Michurin

Some of the most eminent physicists and mathematicians helped biology re-establish itself as a modern science and Igor Tamm was among those at the top of the list.

My interest in genetics and related sciences facilitated understanding with these people. I have particular recollections of the regrettably infrequent discussions on general matters of methodology and philosophy that I had with Tamm during my visits to Moscow. They were in the same spirit of high-quality free debate so typical of Bohr's scientific community in Copenhagen. Personal contact with Tamm was of immense personal significance to me. It stimulated scientific activity, calling up memories of past contacts with leading theorists. Tamm was a scientist of such stature. And I earnestly hoped that talking to a biologist would be interesting and useful to Tamm as a man who had devoted much effort to the development of biology as a science.

I'll not go into what we talked about, but would like to mention what I consider an important joint undertaking in Moscow at the outset of the new stage in the development of genetics in this country.

I met Tamm soon after I came to Moscow in the winter of 1955—56. He told me of plans to devote one of the "kapichniks" (seminars at the Institute of Physics sponsored by Pyotr Kapitza) to modern genetics. Tamm was intrigued by the new concept evolved by Crick and Watson and their associates on the double helix of desoxyribonucleic acid as the basis of the morphology and reproduction of chromosomes, the theory that formed the starting point for modern molecular genetics. He said he would make

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biology and creative Darwinism. After the 1948 session of the V. I. Lenin All-Union Agricultural Academy, the issue became particularly acute, and research in several fields of general biology came to a complete halt. This created an environment for the spread of unverified facts and hypotheses (the doctrine of the noncellular "living substance", the intermittent "generation" of species by leaps, the "transformation" of viruses into bacteria, and so on). Discussions on physiology (combined session of the Academy of Sciences and Academy of Medical Sciences of the USSR, 1950) and the evolutionary morphology (1953) also had an adverse effect. All of this severely hampered the development of genetics, evolutionary theory, molecular biology, cytology, evolutionary morphology, taxonomy, and other branches of biology in the USSR. The situation was completely normalized in October 1964, when steps were taken to restore and promote genetics and other disciplines (corresponding institutes were created, the All-Union Society of Genetics and Breeders was organized, and the training of specialists in these fields was stepped up). This ensured the active participation of Soviet biologists in the rapid growth of world science, in which biology forms the vanguard, during the second half of the 20th century".— *Great Soviet Encyclopedia.* N. Y., Macmillan, Vol. 3, p 318 — *Ed.*

a report on it at the "kapichnik" and asked me to give a rundown on radiation genetics and mutation mechanisms. This was approved by Kapitza and both reports were on the agenda of the first "kapichnik" in January.

A description of some events in the runup to the meeting is well in order. Three days or so before the meeting, when it had already been advertised, somebody called up the Institute to tell Kapitza's aide that the genetics reports should be taken off the agenda as conflicting with the 1948 session of the Academy of Agricultural Sciences. Kapitza said the call should be ignored. The following day there was another call, this time with reference to the opinion of a top official. When Kapitza rang up that official, the latter assured him he knew nothing about it and that the choice of programme for the seminars was solely the director's business. At that the seminar went ahead without further ado.

That seminar at Kapitza's Institute was the first scientific gathering on modern genetics in years. Neither of the two lectures could be defined as particularly militant. In essence, they were conventional papers, prepared to the best of our ability, on two general subjects of genetics: mine summed up progress in radiation genetics, and Tamm highlighted research opening up new prospects for genetics and cytology. Even so, the seminar had implications reaching well beyond the biological community of Moscow. The conference-room, the hall and the staircase leading to it were cram-packed. The Institute's staff, stunned by the influx of people, hurriedly put up loudspeakers for those who could not be accommodated in the conference-room. I don't think the acclaim was due to any special talents displayed by Tamm, the less so by me. Simply, the scientific community, particularly young scientists, had been pining for information in that area for too long.

Our joint address played a real part in the revival of biology. As I have already said, the key role in this process, regrettably, belonged to physicists and mathematicians rather than biologists. The fact that Kapitza allowed genetics to be discussed at his seminar and that a leading theorist like Tamm took part in it, got genetics off the ground as a science and won public recognition for it. As a serious precedent the seminar did much to accelerate biological research.

I would like to conclude by noting Tamm's charisma which was noted even by those who had fleeting or rare contacts with him. Moreover, Tamm was a man who inspired absolute trust. In every way he was a gentleman. Nor could it have been otherwise: all scientists of stature, despite different tastes, characters and interests, are charming and whole people. In my memory

Tamm is kept among those inordinately endowed persons of temperament and many talents who were also great scientists, like Einstein, Bohr, Rutherford, Dirac and Schrödinger.

L. I. Vernsky

IN AND OUTSIDE HIS STUDY

(From Conversations with Grandfather and from the Family Archives)

> And never for a single moment
> Betray your true self or pretend.
>
> — *Boris Pasternak*

Igor Tamm was my grandfather ... It has been my happy lot to have lived close to him. My first seven years of daily contact with my grandfather have left an image graven in my memory that is very dear to me. He was, without doubt, the most respected member of the family and enjoyed the greatest authority. But the Granddad of my childhood was also the most uninhibited, merry and agile of us all. I liked everything about him — he was just the model a child needs. As I grew older, he came to be more than a grandfather, a friend. He became a teacher who gave advice and instruction on a host of subjects. I spent more time in his than in any other company. Those were happy years.

My grandfather had special qualities which was what made him the man he was. He was very generous and tactful, always enthusiastic about his work and genuinely interested in everything new or unknown to him. And two more things: he was a true friend and considerate and attentive to all in his company. Not knowing the world I took these features for granted.

Best of all I remember conversations with him and the stories he told me in the years 1963 to 1971. Tamm always judged others by his own yardstick. So when I was 13 or 14 I became an adult in his eyes. Our conversations became serious from that time on. Grandfather was interested in everything that attracted my attention, or was otherwise very much on my mind. He was

to me a just arbiter, a giver of unbiased judgements, assessments and support, if help was asked for or needed. If in trouble, I would ask nobody's advice but his. He always had plenty of time for me and it is only natural that some of his interests have "rubbed off" on me. I became interested in events of ancient times, tragic and epoch-making periods in history and science. Grandfather's life was very active and packed with a wide range of pursuits. He had embarked on quite a few adventures and did a lot of travelling. His stories I found absorbing. Recalling those days, I now realise that my contacts with him were the highlight of my life then. His illness made me sometimes really worried, but before long the illness itself, bad as it was, became something I barely noticed: grandfather and I talked as much as in the past, or even more. The only change was that he was confined to bed, a curse for a man of his temperament, but he took it with courage and endurance.

Somewhere around 1968 Tamm learnt that a biography of him was to be published. He was very upset by the news. He found the idea of a biography while he was still alive alien. And in his habitual way of dealing with things objectionable or unpleasant, he swept the idea under the carpet and let it rest there. However, before long he was again approached on the subject (this time by Novosti Press Agency).[1] Tamm was very upset and at a loss what to do. He thought there might be a way out, that he could somehow reject the offer, for high honour though it was, to him it was unpleasant. He sought our advice on the matter. Our family and his close friends rejected the easy way out: the problem would simply go away if ignored. "You don't want them to write goodness knows what about you in print, do you?" A writer friend was suggested, but Tamm

[1] A British publisher was planning a series of biographies of Nobel Prize winners. In 1968 they approached Novosti Press Agency for assistance. Later, in 1971 after Tamm's death, Novosti received a second letter from the publisher with a request to recommend a Soviet author to compile Tamm's biography for them with the accent on the scientific aspects. The request was forwarded to the Theoretical Physics Department of the Lebedev Physical Institute where B. M. Bolotovsky, D. Sc., was chosen to do the job. A contract was signed with the Agency. Later B. M. Bolotovsky was joined by V. Y. Frenkel and they began to collect material. (Bolotovsky had interviewed Tamm several times on his research in general, and on the history of the theory of the Vavilov-Cherenkov effect in particular; Frenkel, who was busy compiling Y. I. Frenkel's and P. S. Ehrenfest's biographies, had interviewed Tamm with the same aim in view). As yet the two authors have published only one biographical sketch in the collection: *Igor Evgenievich Tamm*. Moscow, Znaniye Publishers, 1973 (in Russian) — *Ed.*

refused. Then Frenkel was approached. He had written a long biography of his father Y. I. Frenkel, and Tamm had spoken highly of the book.

It was Frenkel, if I am not mistaken, who finally talked Tamm into giving his consent: since it was impossible to avoid the whole thing — biographies of all the Nobel Prize winners were going to be published anyway — it was better at least to get the facts right. So Tamm agreed, albeit reluctantly. That evening grandfather quite unexpectedly suggested possibly I might like to write his life story. "Any desire to do it?" he asked me.

The suggestion nonplussed me. On the previous day I had argued with grandfather that he should lend a helping hand to those who would be writing his biography. The offer really attracted me, and without much ado I agreed to it. Then doubts set in — was I not too young for such a serious and time-consuming job? But grandfather encouraged me as was his way. Just a few hours before others had had to talk him into helping with the project, but now that the decision had been taken, he was going to roll up his sleeves.

"You'll find it easier than anybody else — I have already told you a lot ... Some of my memories are most likely now planted in your mind much better than in mine ... And if you've forgotten something, go ahead and ask ... You and I will remember it together, clear up things if necessary. And I'll find it much easier to go over my life with you than with other people."

We made up our minds: we would do a bit together every day. We should work chronologically as if making up an outline of succession and substance. Things I had never heard of before would surface, and grandfather would tell me about them in detail. At the same time I began to sort out some of his private papers. Tamm had never kept any archives in the strict sense of the word — the idea that this could be worth doing had never even occurred to him. But archives of sorts had somehow formed on their own accord. There were very many letters of varying degrees of usefulness he had received; there were some draft copies of letters he had written; many note-books he had kept; and just scraps of paper with his notes. Grandfather joked: "At long last I've got my own private secretary." He treated whatever I did in earnest and was exacting. Unfortunately, our sessions were not to be regular as his illness took its own...

Years have gone by, but even now I just have to think of grandfather and his stories come back to me. And among his papers and letters I have found things to supplement what I heard. "No archives for me, no thank you. Who needs them anyhow?" he used to say, and no archives there were. Only a wild collection of papers, manuscripts and letters. I found them all over the place — on the desk, the side table, the window-sills, in desk drawers, and in between the books on his shelves. What was done or received today was superimposed upon yesterday's papers, while the sheets of the-day-before-yesterday were lost. When he travelled, his study was more or less put in order, or when we moved to the summer cottage and back home, the old notes and manuscripts were shuffled about and tied into bundles. So some papers would surface, while others would disappear into the built-in book-case, or be pushed into the attic. Some of them were thrown out together with torn up letters, draft copies and criss-crossed sheets of calculations. Sorting out such "archives" is a very engaging pastime, and if the papers belong to your own grandfather the job becomes absorbing. Only things he was working on at that moment were safe from loss or damage. The sheets he needed were always close at hand. Numbered pages of the calculations he was making ("continents" he called them) and rough copies of the verse he wrote ("islands" was his name for these) were floating about in the chaotic paper agglomerations that filled the study.

But the real trouble was this: when you try to commit what you hear to paper you often find yourself up against the obstacle that stories tend to fade in your memory even if you heard them several times. You are left with no more than a vague impression. You feel hopeless — the record seems to be right, and still something is missing or has gone wrong somehow. Then you realise that what has been left out is Tamm himself, his gift of telling a story quite simply, but with much verve and colour! Writing his biography is really difficult. Four-dimensional impressions just will not fit onto a two-dimensional sheet of paper! The life of the narration is lost. Sometimes I listened to other people's reminiscences, but they presented some Edward Lear[2] turned physicist and not grandfather.

The more one knew Tamm, the harder it made writing about him — communication with him was too intense and out of the nor-

[2] A painter and traveller — *Ed.*

mal. Sorting the layers of daily, weekly and monthly correspondence is a mammoth task; they were "condensed" and packed so close. Unforgettable impressions could hardly be broken down into components, or so it usually seemed ... Not impressions, but something more meaningful and important. Very much of what appeared to be your own was, in fact, passed on to you by Tamm...

Tamm's letters sometimes read like digested or fragmented outlines of the stories he told. But in them, too, one finds a wealth of detail and original observation, evidence of his interest in varied aspects of various countries. The space and time for writing were too short, and the abundance of impressions too great.

Tamm's style was quite simple and embraces almost all his stories, letters, articles and notes. Lucidity in everything was his primary aim. Once he remarked that he had always tried "to make his calculations pleasant to look at" and to give them "an elegant" form.[3] Concern for "elegance" seen as clarity and intelligibility guided Tamm in everything he wrote or said. His popular science articles are written in the same lucid and vivid style, as are many of his articles and public lectures on science.

Nearly all his articles and records of many of his public lectures have survived. Not so, however, his correspondence. Most of his letters have been lost or destroyed. Few of the letters to his parents in Kiev written between 1913 and 1941 survived the war. Letters he received in the late 1920s and early 1930s we found among his papers and they indicate extensive correspondence with many Dutch and German theoretical physicists. Some of this correspondence was resumed in the post-war years. He also corresponded with Paul Dirac and the mountaineer Bell, a friendship striked up during his stay in Edinburgh. Later (mainly in the 1950s and the 1960s) he exchanged letters with scientists in Britain, Japan and the United States, among others with the geneticist Lederberg.[4] In the files of his correspondence with Niels Bohr (kept by Tamm's family) there are only three of his letters dated 1936. There are no answers to Bohr's letters of 1932 and 1934. It appears that they are also missing from the Bohr archive in Denmark, as Professor Aage Bohr kindly informed us in a letter of February 2, 1977.

Tamm's relations with most of these correspondents were very friendly. His letters are of interest not only as a source of biographical data; they are of no less value for the history

[3] I. E. Tamm's letter to P. Ehrenfest (February 24, 1930).

[4] Joshua Lederberg, who specialised in the genetics of microorganisms. Lederberg and Tamm got to know each other at the Nobel Prize gathering in 1958 in Stockholm (See I. M. Frank's reminiscences in this book).

of science. It is discouraging that his correspondence has not all yet been collected and that some of the letters have been lost never to be recovered.

This is especially true of Tamm's correspondence with his foreign colleagues in the pre-war years. In August 1970, when Tamm was already very ill, Victor Weisskopf visited him at his summer place. Recalling the past, Weisskopf told some of Tamm's students who happened to be there that after Hitler came to power in Germany he had been forced to leave that country and had settled in Zürich where he worked with Pauli. The Swiss authorities at that time were cringing before Hitler, and the correspondence of all those on the "suspect list" was opened without scruples and inspected by the police. One day in 1934 Weisskopf was summoned by the police. They showed him a letter to him with an invitation from Tamm to come to the USSR and stay at the Tamm's new four-room flat. "Only top-line Communists have good flats in the Soviet Union, so you must have close links with Soviet Communists. You must leave Swiss soil at once and you will never be permitted to return," the police official told him. It was only with great trouble that Pauli managed to get the deportation order suspended for a month to allow Weisskopf to finish his course of lectures.

Weisskopf became a naturalised US citizen and 33 years later was invited to the European Centre of Nuclear Studies (CERN) on the outskirts of Geneva to fill the post of Director General and Scientific Leader. On arrival, he visited the police headquarters and reminded them of the ban in perpetuity. So what was he to do? The official he spoke to could do nothing but try to smile off the whole incident and ask Weisskopf to let bygones be bygones. I might mention in passing that in the ensuing five years Weisskopf headed CERN with efficiency and distinction.

"Agraphia", to which Tamm was often fond of referring, set in in 1958 when he began to receive so many letters that it was physically impossible to answer them all.

Now all the papers have been sorted out. Most of the letters he received from abroad have been translated into Russian. Some of the letters he dispatched abroad have been uncovered and sent over to us. There is hope that I shall make good my promise to grandfather that I would write his biography. What I contribute to the present collection are not excerpts from the future book. They are just stories I heard from grandfather, a few documents, and in my view characteristic episodes, my fragmentary reminiscences of the man and thoughts evoked by his wonderful life.

* * *

Two quite dissimilar images of Tamm emerge in my memory. The first one, grandfather in his study, and the second, grandfather outside it. The first and the second images are in diametrically opposed states: almost complete immobility and perpetual motion. In my mind's eye I see him in a chair at his writing desk, his back to the door, his body still, bent over a sheet of paper he is writing on with a fountain-pen. That is what I saw for many years when I peeped into the study. One could stand in the doorway for five or ten minutes without seeing him move his body at all, except for his hand. Once in a while his writing would slow down, only to pick up again. That was the only movement one could see. Then the sheet of paper would be turned over or a new one reached for from the stack. This done, complete immobility of the back, shoulders and head would set in again. Sometimes the arms would swing up abruptly: what had just been put down on paper would be crossed out. When that happened, the last formula before the crossed out piece would be carried over to a new sheet. Then one would hear the striking of a match, and the left hand would come into view with a smoking cigarette. I would find grandfather Gora (a diminutive of Igor — *Ed.*) busy working in the afternoon and late at night. Sometimes the streak of light under the study door could be seen into the small hours. Grandfather would usually work 12 to 14 hours a day. The pages of his calculations were numbered in astronomical figures. More then once during the discussions of what creativity was all about, I heard grandfather say with profound conviction that both the worker in the arts and the scientist needed more than ability or "sheer" talent, that a strong will and hard work were imperative. He was fond of Edison's definition of genius: "Ten percent inspiration, ninety percent perspiration." "Do you get it? Even a man of genius needs to work at it!"

He believed that there were very few men of that category in science: Newton, Faraday, Mendeleev, Einstein, Bohr and Dirac.

To visualise Tamm standing in one spot outside his study would overtax the imagination. Only when moving about was he in his element. His whole body was in a state of permanent flux. Photos of him were almost invariably poor: his mobility was lost in them; a face that constantly changed in expression became unnaturally strict and stiff; his specific gestures came out strange or were simply lost altogether. Exposure time, no matter how short, seemed to him abhorent and unbearably long. When he sat for a painted or sculptured portrait, the results

were even more deplorable. Only caricatures done by a friendly hand and exaggerating his movements bore some resemblance to the original. Free from work, he would fill each moment of leisure by moving about. When he received guests or spoke on the telephone he would pace the room (though the telephone flex limited his range). He would pace to and fro in the room when thinking; sometimes the room was not enough and he would take in the corridor. When a heavy cloud of tobacco smoke formed in the room Tamm would go out onto the balcony, or down the stairs, around the house and along the embankment from one bridge to the next. In summer at our country cottage he would walk around the village two or three times, or sometimes go to the nearest forest. Strolling was a passion with him as walking did not interfere with his thoughts.

Everything Tamm did he did nimbly. His movements, gestures and mime were all harmonious. His motion never got at anybody but induced others to the same.

I can still see him telling us something at table, and the cutlery would often be called into play to illustrate the story. Even the salt-cellar, the bread-basket and the cigarette case would go into action. He could talk on a host of subjects: the latest experiments on mice, telepathy, the abominable snowman. He would talk willingly to anybody, and always sought to know their passion in life.

Had he himself any interests outside physics? My answer would be: almost everything.

I admired his range of interest, it was so very wide. There were only two lines of human activity he was always rather indifferent to: music[5] and "pure" technology. Memories of his music lessons always made him smile. In his first school years his parents arranged piano lessons for him. Tamm's mother Olga Mikhailovna was very musical and invited a first class teacher. In his teacher's view the boy made some progress, though he did not show the tiniest shred of enthusiasm for his mother's idea. Once Olga Mikhailovna listened from the next room to young Igor playing during the lesson. She liked what she heard, and especially one of Mendelssohn's songs her son played. When the teacher left, she asked Igor to play the song over once again for her.

"Which one?"

"This one," and she hummed the tune. Igor frowned:

"But I've never heard it before, I've never heard it and never played it."

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[5] For "musical moments" in Tamm's life see the reminiscences of V. D. Konen in this book — *Ed.*

Olga Mikhailovna was taken aback. She went up to the music rack and found the score of one of Mendelssohn's *Songs Without Words* in it. Her son played it without a single mistake. Then the perplexed mother unravelled the mystery: young Gora had no ear for music at all. He was using his visual memory: he knew which keys to strike and when. And that was so with all the pieces he had learnt! To play a piece of music without having seen how the keys had to be struck was completely beyond him... that was the end of his piano lessons.

In later years, though his wife Natalia Vasilievna was a very musical person, he never went to concerts. But they went together to the opera — way back when they were high school pupils at Elizavetgrad, and when they studied at Moscow University. *The Queen of Spades* by Tchaikovsky was Tamm's life-time favourite. He liked songs: Ukrainian folk songs he had heard in his childhood and youth; songs of revolution (the *Warshavyanka* and the *Marseillaise*); the bard's songs of the early 1960s. What attracted him were the lyrics. When listening long to instrumental music he became impatient, but when it was accompaniment to some action or a tune to some lyrics, his attention was captured.

The theatre, poetry and literature were all part of Tamm's life. He could not draw very well himself, but he was enthusiastic about art. He liked sculpture and architecture, and enjoyed amateur theatricals and charades. His ability to "build a character" and fit himself into the scene of action is well illustrated by the following incident: in one game of charades Tamm and Boris Smirnov (an engineer and a friend from pre-Revolution days) had to act getting into an overcrowded tram. A do-it-yourself bookshelf with the two rows of an 80-volume encyclopaedia hung on a wall of the largest room, which was where charades were usually staged. Like passengers trying to get hold of the handrails of a packed tram, they grasped the shelf at both ends, and all 80 volumes came crashing down. The two actors had got totally carried away.

I still remember his caricatures, though I last saw him perform them in the late 1950s. And it was also exciting to hear his stories which often turned into one-man theatre.

I remember an exciting game grandfather suggested in 1956 to his grandsons to be played out on Nikolina Hill. First he told us the story of the adventures of Shamil[6] who had found refuge deep in a forest near Moscow. The game began, as

[6] Shamil (1799—1871) was a Muslim leader in Daghestan, led a liberation struggle of the Caucasus high-landers against the tsarist colonialists and local feudals.

might be expected, with a cryptic coded note found in the hollow of an old, rotten tree-trunk during one of our walks in the forest. The note was decoded by two grandsons and grandfather Gora. At long last the message was read and understood. We were quite unaware that it was grandfather who had written the note. The text was signed "Shamil". Then came the fantastic story which we, of course, believed. The correspondence continued, the codes were changed, each message informed us where to look for the next message and when, usually after dark. Looking for a message, we grandsons had to train our observation ability and ingenuity, show our manliness, and decoding was a real brain-teaser. We were absolutely delighted with the game, and it gave grandfather himself a good deal of pleasure. The summer came to an end and we moved to another cottage, but our "correspondence with Shamil" went on for several years and grandfather's real part in it was never discovered.

The stock of exciting games we played with grandfather was great. He liked them to be original and to require quick wits, courage and ingenuity in dealing with difficulties, qualities he wanted to see in us. But where did he find the time for all this? An incredibly intensive worker, he needed a pastime as active as his long working hours. He always got completely involved in anything he did and this made any game one played with him intriguing. He never lectured us or acted as our mentor: he behaved as our equal, and his resourcefulness and inventiveness were fantastic. It wasn't only we his grandsons who were drawn to him, but all the children around, all of whom loved him dearly. His memory for funny, entertaining stories was inexhaustible, but tales of horror were his favourites. Children and adults alike listened eagerly. On his return from one of his many trips grandfather would for several days at teatime treat us to impressions, verbal sketches, legends and accounts of things that had happened or which he had heard from people with him or whom he had met along the way. Quest for knowledge, and sociability were part of Tamm's nature. Some of his letters from his travels read like fascinating stories. Descriptions of towns and scenery flow with word pictures of the people he met and remarks on their manners and life styles. They are full of adventures and stories about eruptions of volcanoes, brave seamen and small boys kidnapped by bears...

* * *

The doorbell rings: a man in a fearful mask appears on the threshold. The face is beastly with popping eyes and a coarse

tongue hanging out, and a skull and crossbones painted on the brow; large gilded ear-rings dangle from his ear-lobes... Grand-dad Gora reels with laughter carefully putting the mask down. Suitcases flank him.

"I had it on throughout the flight. It's very fragile, you know. I was afraid to pack it away in a suitcase — it could have been broken during handling. It's a ritual mask from Tibet. I talked myself hoarse convincing them to sell it. Isn't it wonderful? I took to it the moment I saw it."

The mask was hung over the door in the study. When you were coming home in the evening and turned into the yard, you could see a bright red patch over the door of the study if the lights were on in the room. That was the mask.

Tamm very often recited outloud some verse he liked. It might be a long piece of poetry or just a few stansas, or a few lines from a song. In his papers and notes there were always quite a few verses, aphorisms or proverbs he had copied down to help him memorise them. I shall quote some of the notes grandfather made in the forties, fifties and sixties.

Entries made in the manuscripts titled "Hydrodynamics of Heavy Nuclei" of February 23, 1943 and "Sound in a Shallow Water Body" of April 1, 1943 read:

"Science was progressing in defiance of Hippocrates' well-known aphorism: 'Art is long, life is short, experiments are dangerous, reasoning is faulty.'"

"A French philosopher of the end of the 18th century said: 'Sans théorie on ne sait ni qu'on dit quand on parle, ni qu'on fait quand on agit'" ('Without theory one does not know what he says when he speaks nor what he does when he acts').

A quotation from a notebook kept from 1932 to 1954: "A pupil is not a vessel to be filled, but a torch to be set alight!"

A quotation from the last notebook he carried with him from 1958 to 1964: "New kilometres behind you make you richer, though not kilometres covered thoughtlessly. A road without a destination leads nowhere."

All his life Tamm periodically wrote poetry himself. Mostly for children or special occasions and humorous and lyrical verse. He never took it seriously, and many of these compositions have been lost. As I go through his papers and come across verse in his "sharp" handwriting, I recall quite clearly the way he read those lines...

He copied out stansas from Omar Khayyam's Rubaiyat more often than verse by any other poet. They were grandfather's favourites. Grandmother liked them, too. Omar Khayyam's

incurable optimism and love of life were in grandfather's style. He liked the aphorism and elegant idiom.

Tamm took great pleasure in reading Omar Khayyam's Rubaiyat in Edward Fitzgerald's English translation and in the Russian translation by I. Tkhorzhevsky, and O. B. Rumer (the latter he knew).

A copy of Anna Akhmatova's *Requiem,* hand-copied by grandfather in the forties, has survived. Of Pasternak's poems he especially liked the last lines of *Marburg:*

So why should I be faint?
Insomnia's my ally,
Like grammar rules I know it...

Tamm interpreted "insomnia" here not as an illness, but as inspired, absorbing work that keeps one awake far into the night!

With the floor in moon light patches
For our board
The nights and I keep playing chess...

These lines were copied again and again and can be found in many places among Tamm's calculations. Chess, night vigils, formulae and optimism: those were just him. In a tight corner in a game of chess, or a crisis, winning the game:

The poplar is King. Insomnia is my partner.
Nightingale is the Queen. For her I reach...

Tamm used to compare Pasternak's verse with Lewis Carroll's *Alice Through the Looking-Glass* — he probably had Pasternak's early poetry in mind. Pasternak's later works attracted grandfather for their lucidity, poetic imagery and an "unheard-of simplicity". I recall his superb recitations of three poems published in an issue of *Yunost* he had just received: *A Tale, The Lord's World* and *August.*[7] Still later in life grandfather liked Pasternak's philosophical poem *To Seek Renown Is Improper* and the biting satire in *Arrogance* by Alexei Tolstoy (a 19th century Russian poet — *Ed.*). For many of his closest friends the poem *To Seek Renown* is still associated with my grandfather. This verse was quoted by his students in an article on Tamm's life and research.[8] And *Arrogance* expressed an attitude that was pure Tamm. The two poems are like two horns of a dilemma.

[7] *Yunost*, 1965, No. 8, pp. 70—71.

[8] When Tamm read the verse in this birthday article, he interpreted it as applied to him in a different way. The meaning put by Pasternak into these lines moved him so much that he rushed to the authors of the article for explanation. "It reads: 'But you yourself must not distinguish your victory from your defeat.' How's that?

Alexei Tolstoy and Lermontov were Tamm's favourite poets even in childhood. He was also fond of Kotlyarevsky (a Ukrainian poet — *Ed.*) and knew long passages of Kotlyarevsky's *Aeneid* by heart. A large volume of *Complete Works* of Alexei Tolstoy, a present to his mother when she passed her finals at the Pavlov Institute, was worn and shabby with much reading, and it had been patched many a time with loving care. Grandfather also liked N. Gumilev, O. Mandelstam, E. Bagritsky, and D. Kedrin (20th Century Russian poets — *Ed.*). He was always interested in poetry on a science theme. Poet Y. Zimin, for one, was a friend of his.

Tamm read English, French and German poetry in the original. He would turn to Burns, Shakespeare, Heine, Schiller, Poe, Baudelaire and Whitman more often than to other poets. Sometimes he would wish to restore something that was slipping his memory. At the end of 1967 I used to visit him regularly in hospital and we often discussed poetry. We wanted to go over Schiller's *Glove*, but neither he nor I remembered anything but the first line of the poem. Copying it was a priority before my next visit to him. I remember the trouble it cost me, for the only copy I could find in my hurry was in the Gothic print that I hated, and the poem is rather long. A year or two later it was the *Marseillaise*. We had no copy of the French at home, but soon grandfather was very happy for friends unearthed not one, but two copies for him.

The longer he had to rest and not work (grandfather without work!!!), the more time he had for reading and various "trifles". When his illness was at its worst and he was bedridden for days on end, he used to amuse himself by making up verses in a German folklore style. He made up plenty of them. The Ukraine of his boyhood inspired him to write verse in Ukrainian. He made neat copies of these poems and presented them to my grandmother.

Reading fiction, poetry and the latest Agatha Christie and Simenon books was a daily pastime of Tamm's. He was a quick reader, but if he read a detective story he liked to enjoy it longer. He read a lot on archaeology, history, sociology and biology,

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What do you think?" Subsequently, it was my impression that those lines about victories and failures were very much in tune with Tamm's views on creativity. Work, any work, stands an equal chance to be a success or a failure. Neither of these outcomes would make him stop or give in, i. e. success or no success he would keep on, though a failure would make double his enegry. It was so in Holland in 1928 when his search of many months flopped, and in the last years of his life when a theory he worked at just would not come. A success would be followed by a new effort, a new offensive.

the subjects he was interested in. All new and unknown things attracted him, and his interests covered a wide range of subjects.

Mountain climbing was yet another of his hobbies. Forty years after, grandfather was still telling of his first, rather undistinguished climb. In the summer of 1926 he had gone to the Altai with a group of younger climbers (students and postgraduates, some of them some 10 years younger than Tamm): M. A. Leontovich (the theoretical physicist), P. S. Novikov (the mathematician), N. N. Pariysky (the astronomer) and his wife L. V. Pariyskaya,[9] E. P. Sveshnikova and the composer M. L. Starokadomsky. They were a fine, closely-knit group. (In later years many of them formed hiking and mountain climbing teams).

Probably Tamm's greatest personal achievement in handling machinery was learning to drive a car which he did under the supervision of Paul Dirac. The latter owned the car in which the two of them toured England and Scotland in 1931. When the trip was over, Dirac talked Tamm into taking a driving test in Cambridge. Grandfather was always proud to have got his driving licence in Cambridge. But in the following 18 years he never drove a car (though in the thirties his younger brother Leonid Tamm owned a car he had received as a prize from Ordzhonikidze). In 1949 Tamm bought a "Pobeda" car and brushed up his driving with the help of his son. He drove it several times and did it with obvious pride and pleasure.

While in Scotland, driving lessons alternated with rock climbing. Tamm did a solo climb on a route that had been tried only once before. It was an exceptionally difficult (grade 4) climb on the Isle of Skye in the Hebrides.[10] He was still a beginner, for the little rock-climbing he had done in Germany in 1928 was in his view hardly worth counting. He had made three ascents in the Central Caucasus: the Fitnargin Summit (the first climb with Tamm as the team leader) in 1929; the Tyutyurgu-basha (the first climb); and the Bashil-tau (the first Soviet climb over a pioneering route along the North-Eastern Range) in 1930. It is interesting to note that Dirac only liked climbing mountains with Tamm as a companion. During Dirac's later visits to the Soviet Union grandfather never gave up trying to "win him over to mountain climbing". Here is a curious excerpt from a letter by K. K. Tikhonov to Tamm dated February 5, 1967: "It is summer 1936, the Caucasus, Mount Elbrus. A group of students from the Moscow

[9] See reminiscences by L. V. Pariyskaya in this book — *Ed.*

[10] *Adventure in the Mountains: an Almanac.* Book 1. M., Fizkultura i sport Publishers, 1961, p. 45 (in Russian).

Transport Engineering Institute led by me was joined by the Soviet physicist Tamm and the British physicist Dirac. After two days of acclimatising at the 'Eleven Mountaineers' Shelter' we scaled the eastern side of the Elbrus ... I recall our futile attempts to get the 'USSR Alpine Climber' badge for Dirac for having done that ascent ... But what if we try to get it for Dirac now, 30 years after? ... Several years ago I received a short note of greetings from Dirac: he remembers that climb..."

Apart from driving, Tamm's record with machinery was modest: when his daughter and son were small, he used to mend fuses when required and would do it, if asked, for the neighbours. Curiously enough, Tamm, the author of a textbook on the theory of electricity, treated household wiring and appliances with mistrust and apprehension all his life. My explanation for this is poor experience — he belonged to the rather sizeable group of theoretical physicists whose occasional attempts at physics experiments always failed.[11] He was very much "drawn" to experimenting, but in the mid-1920s he gave it up for good.

His interest in archaeology, too, was not restricted to reading about finds. His interest in new finds, and sometimes even a thirst for miracles, danger and adventure, prompted him to enlist in expeditions.[12] The quest for the legendary treasures of Queen Tamara, and the mysterious ruins of an old monastery, probably the one mentioned by Prince Vakhushti in his *Geography of Georgia*, tempted his curiosity.

In the 1960s he noticed several hillocks near his summer cottage that looked like burial-mounds. He made up his mind to get a permit to dig them up.

All his passions were infectious. I, for one, owe my interest in archaeology to grandfather.

Tamm was fond of reading aloud. He preferred the *Scientific American* to other popular publications. Incidentally, his linguistic ability was magnificent. In his after-dinner relaxations that lasted half an hour or so he would often read for grandmother novels, detective stories and articles. He would read and translate into Russian at sight from English, French or German; his delivery was fluent with only very occasional short hesitations to find the right word. When he read for children, the manner was different: he would take in the whole page, or even a short chapter at a glance and retell it, in Russian of course, which was much more fun than "direct" reading. When he was gifted Ada Negri's book, he

[11] See also reminiscences by S. M. Rytov in this book — *Ed.*

[12] See reminiscences by E. A. Kazakova in this book — *Ed.*

set about learning Italian. There were only Italian-German dictionaries at home and a very thin teach-yourself Italian textbook. Yet before long he began to read the book to us in a really surprising manner: translating it into Russian at sight he would improvise assonance rhymes. The Latin he had studied as a high school boy helped him with Italian, as he once told us with great satisfaction.

Granddad would never miss a chance to play a game or two of chess. Apart from reading, playing chess was his favourite "static" pastime. Marina Tsvetaeva once defined playing chess as an "endeavour directly opposite to composing verse." Accepting this definition, I wish to point out three reasons for grandfather's infatuation with the game. First, it occupies one's attention and stops one thinking about other things. This gives the intellect active rest. Second, one can build up an elegant combination that gives aesthetic pleasure. Third, it is an exciting game not based on chance, a contest of brains. His pensive great attack planning often ended in a need for desperate defence after failing to see what his partner was up to. And when grandfather was beaten he would demand a return game. He would want another go at using the plan his partner had not let him use in the previous bout. And if he won, he would ask merrily: "Well, well ... What now? A return match?"

Two traits of Tamm's nature came out in sport: manliness and a child-like attitude with keenness on the game linking the two. Mountain climbing, scuba diving, riding, skiing. His main demand on any sport was that there should be as much movement as possible! So there was also rowing, tennis and volleyball.

In the summer of 1954[13] grandfather spent his holidays in the Altai area. An intriguing certificate connected with the holidays has survived among his papers:

"This is to certify that Comrade I. E. Tamm during his training at the Nauka Ak-Tru Alpine Camp (North-Chuya Range) from July 13 till August 2, 1954 ascended the Tamm Summit on July 31 (difficulty rating — 1b; route — snow-coated slopes)."[14]

On the way home to Moscow Tamm used local services through Biyisk to get to Novosibirsk. At Novosibirsk Airport the queue for tickets was huge; there were people milling everywhere. Tamm was in a great hurry and decided to flash his identity card of Hero of Socialist Labour and ask if he was entitled to skip the line. The official had a look at him — a tanned, weatherbeaten figure,

[13] The title Hero of Socialist Labour was conferred upon him that year.

[14] It is a banter: 1b follows the easiest 1a ascent; Tamm made 3b ascents.

unshaved, grey-haired, wearing a creased checked shirt and coarse boots, and with a worn, faded rucksack on his back (that was normal for a tourist then when there was little tourism). Then he answered: "Sure Pop. Can't you read or something? Let's read that sign there out for you..." Tamm related with great relish how he had obviously been taken for a seasoned shepherd.

* * *

Tamm often used to remark that a human personality, convictions and outlook take final shape early in life. He believed that human traits, moral standards and basic interests could change but little throughout one's lifetime, and that once the basic features of a person are formed they remain with him for life. Such convictions, undoubtedly, stem from his experience, and this makes all the more interesting reading the notes Tamm made during the last years at high school and while a university student in Edinburgh and Moscow. And much of what he says in those notes is confirmed by his way of living. I shall alternate some of the stories told by grandfather with short excerpts from his rare diary entries. After 1914 he never kept a regular diary, merely copying out passages and aphorisms he liked from the books he read, sometimes adding his own comments. Before the 1930s he used exercise books for the purpose, and later on, special notebooks. Such entries, though always fragmentary, are indicative of his convictions and interests.

Tamm recalled that his first denial of God came when he was a high school junior. So he began playing truant from scripture classes. He got away with it for quite a while: it transpired later that the priest in charge of the class had taken Tamm for a Protestant and the non-Orthodox pupils were not supposed to attend such classes. When Tamm's father was asked to visit the high school for some other reason, the truth about the family's religion came out accidentally. Igor had to go to the hated scripture classes or be expelled from school. For that, the son's atheism ceased to be a secret from the family. "My parents raised the subject with me only once," Tamm recalled, "when they told me that I had the right to my convictions, but if I wanted to get through school, I would have to go to scripture classes. Otherwise I risked getting a 'marked certificate' blacklisting me and preventing me from attending any school at all."

Here is an excerpt from Tamm's report card after the seventh form, the last but one at Elizavetgrad High School. "1911/1912

school year. Marks for the year: Russian 3; Latin 3;[15] Algebra 5; Trigonometry 4; Physics 4; History 4; German 4; French 4; Conduct in class 4. Decision of the Teachers' Council: moved up to the next class." A note on the reverse side of the card reads: "Second Term: tried to avoid attending morning prayers and church service; frequently late to class. Third Term: frequently late to morning prayers (31), breach of order; reading books in class..." Five years had passed since the first conflict with the priest in charge of the scripture class. The threat of expulsion was past, so Igor got very casual about attending prayers which he felt were senseless.

* * *

Here is an excerpt the 13-year old Igor Tamm copied from *On to the Stars* by L. Andreev:

"(Treich): We must move forward. Here they spoke about defeats, but there are none. I only have victories. Earth is like wax in man's hands. One has to squeeze and mould it and create new forms ... But we must move forward. If a wall gets in our way, it must be destroyed. If a mountain blocks the passage, it must be removed. If it is a precipice, we must fly over it. If we have no wings, we must make them ... If the sky comes tumbling down on our heads, we must stretch our arms up and pull the sky out of the way. But while the sun shines, we must move on forward.

"(Lunz): The sun will die, Treich!

"(Treich): Then a new sun must be lit... And while it shines, we must move on forward. Comrades, the sun is also a proletarian!"[16] (Copied out in March, 1909).

Entries in the "School Pupils' Day Book for the 1909—1910 School Year" made by Igor Tamm, 5th form at Elizavetgrad High School:

"Ontogenesis is a repetition of phylogeny only in an abbreviated form (*E. Haeckel.* Biogenetics)."

"Psychological phenomena in the Protista domain bridge the gap between the non-organic chemical processes and the spiritual life of the higher animals (*Verworn.* General Physiology)."

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[15] What a 3 for Latin stood for becomes clear after reading F. V. Nikitin's reminiscences in this book — *Ed.*

[16] A curious slip of the pen young Tamm made: the original says the "worker" (see: L. Andreev. *Selected Works.* M., 1959, p. 55). This is the earliest evidence that Tamm had read Marxist literature — *Ed.*

Here are two epigraphs, and the beginning of an article Tamm wrote in 1909 at the age of 14:

"Prison is the only place fit for an honest man in present-day Russia (Leo Tolstoy)."

"A war is sacred when fought for freedom (Lord Byron)."

"November 6. I know that many will think my article out of tune with our times. But I am sure that this view is wrong. The social and political ideas of the intelligentsia and of the conscious role of the masses displayed on the eve of the 1905 Revolution are rooted in antiquity ..."

"November 10. ... They said and still are saying that enthusiasm for socialism, as socialism itself, is a thing of the past. Is that true? One might well say that the doctrine of historical economism (materialism) has now gained general recognition..."

"November 11. Everybody now will have to agree that the knell has struck for capitalism in economically advanced countries. And that its death agony has already begun, though the throes will, in all probability, last for a very long time yet."

"November 11, 1909. Was it not progress when the Earth became transformed from a mass of condensed vapours into a ball coated with a cooled solid crust? In evolving through the biophores, monera, ameba, simple organisms and coelenterates stages etc., and advancing to Mammals and *Homo Sapiens*, has organised matter not made progress? But *Homo Sapiens* only appeared some two million years ago[17]— a very negligible span of time in the history of the Earth ..."

"The success of socialism may be seen as a progressive development — the form of social organisation that will supersede capitalism will be socialist society" (1909—1910).

"(Autumn 1910) ... In the spring of the current year of 1910 I stood at the front in Latin class, reading Jules Guesde's pamphlet *Collectivism*. I was impressed by this idea in it. At the dawn of humanity everybody did manual labour. In ancient Oriental kingdoms, Greece and Rome the slaves did back-breaking work all their lives, allowing their masters to advance culturally. In the Middle Ages the job of the slaves was done by serfs, and in modern times it is done by the proletariat. In the future the

[17] It is worthy of note that before the 1950s and the 1960s the appearance of *Homo Sapiens* was usually considered to go back some 100 thousand years; only at the beginning of the 1970s, after L.S.B. Leakey's finds in East Africa and a corrected attribution of the Java finds, was the dating of *Homo Sapiens* birth shifted back 1.9—2.1 mln years!

machine will slave for man, and this work will enable man to enjoy life and do brain work."

"November 27, 1910. ... Untrimmed materialism is an imperative basis of my ethics — that is negation of any deity, of the immortal soul, or, otherwise, the reduction of all phenomena to physical and chemical processes."

"May 29, 1911 ... I want (I have always wanted) to be honest in dealing with my own self. Always and without exception."

"March 11, 1912 ... A new definition of life is needed ... As an optimist through and through, I keep hoping that the new way of life which I am so much absorbed in will set in before long ... What will that 'absorbing new way' be like? Science will not satisty me ... I will not be a philistine. So, that leaves only revolution. But will it ('the absorbing new' — *L. V.*) really prove to be *absolutely* absorbing? That is the question. Primacy of an emotion over an idea."

"March 18, 1912. 'Each man forges his own happiness in life,' as the saying goes. The outcome of the forging depends upon the smith, and so a man's life depends upon him, on what he does. However, a proviso is called for here. As a smith cannot forge out of metal everything he might wish, so a man cannot always make his life what he might wish it to be. A person's work is more arduous in all aspects than a smith's. If the latter does not like something he has done, he discards it and has another go. This is not so with man. If one's life is a failure, it cannot be discarded and a new one begun. Life that is gone and vanished is not to be retrieved. It follows that each man at the very start of his life should make up his mind what he intends to do with it. 'Each man forges his own happiness in life.' Each person has to work for his own happiness without waiting for it to come as a blessing from above. But to go that way, one has to have faith in oneself and in one's capabilities. Let happiness be out of our reach, let it not even be true happiness, but the very act of 'making' it brings one happiness. The individual development of a human personality and its endeavour to capture happiness run parallel with humanity's overall desire to improve its lot, though many individuals act to this end quite selflessly, working only for the good of the many to come in the future."

Notes made in 1912 and 1913:

"Before you try to improve others, improve your own self."

"Philosophical systems cannot be built on feelings: no matter how strong a faith, doubts are certain to come up that have to be refuted by the arguments of reason."

"Christianity is falling into ruin because there is too little room for thought in it."

"The basic idea: 'The good, the beautiful and the truth do exist.'"

"Evolution has been and will be everywhere. A human individual, like all humanity, can either slow down or accelerate evolution. Man's mission is to assist evolution. Socially, it must be directed against status quo because 1) it runs counter to the basic idea; and 2) social patterns are already ripe for evolutionary change ..."

"Do what needs to be done now, and do not think about future actions (Leo Tolstoy)."

Why did Tamm go on to Edinburgh University when he left school? I'll tell you what he told me. When a senior high school student, he went in for illegal activities, attended a Marxist club of Elworth factory workers, and took part in meetings and May Day celebrations. Aware of their son's determination and his propensity for resolute action, his parents were very worried for his future. They feared especially that if he entered Moscow or Petersburg University, he would wind up in prison or in exile in Siberia or other far-away places. So they resolved to talk Igor into going abroad for university education. They argued that it would broaden his mind and, on top of science, he would learn another language. So they suggested that he should choose a Scottish rather than a French or German University (French and German were on the school curriculum). They disfavoured London University, arguing that life in "that large city would distract him from serious studies." A more important objection to London University they kept to themselves — in those years London was "seething" with political activity. Revolutionary feelings ran high in the British capital, especially among the numerous Russian political emigrants there. That he should go and study abroad was not an order, but a sincere request from his mother. She confessed she worried very much for his father's health, as he had serious heart trouble. If something happened to Igor because of his political involvement, it could kill his father. Suppressing his urge to go overtly into politics was very hard for Igor, but he succumbed to his mother's request.

In 1913 and 1914 he would go down to London on very short stays where he met Russian political emigrants (he kept this secret from his parents, though such links did not entail any police reprisals). In placid Edinburgh Tamm missed active political involvement. First it was London he wanted to move to and join London University (late 1913 and early 1914); then he had Petersburg and Moscow on his mind (from the spring of 1914). Unable to resist this desire any longer, he sent a letter

to his mother with his *firm* decision to continue his studies in Russia.

There is still another thing that is worth mentioning. In 1913 Tamm entered the Edinburgh University Chemistry Department. In addition to the first year course in chemistry, he took an introductory engineering course (the summer term), the first year mathematics course, chemistry practicals (three terms), and 50 practical physics lab assignments. The course in mathematics was given by the famous Edmund Tailor Whitteker. Soon Tamm signed up for an extra transitional course in mathematics (with exams after each of three terms) and then another three terms maths course. But he did still more (though he found the second year maths rather difficult, as he was to recall with irony). He also attended lectures on English literature, and studied French and German. His final choice to move over to physics was made in the summer of 1914. At the end of May 1914 he received a letter from his mother. Here is an extract from it: "... Very sad thoughts about your future, thoughts that will not leave me night and day (...). Post me an application and an Edinburgh University certificate for the Polytechnic; meanwhile we are getting a copy of your General Certificate of Education ready" (May 13, 1914).

Back in Russia for his summer holidays Tamm was certain that nothing could make him go back to Edinburgh, to "that best city in the world", as he called it on August 30, 1913. At least not to study at the ancient university there! On his way back to Russia he was still in two minds where to continue his studies: at the Polytechnical Institute in Petersburg or Moscow University? He was no longer enthusiastic about moving even to London.

Here are a few excerpts from Tamm's Edinburgh letters to his future wife Natalia, which give some clues as to his outlook on life.

(Undated. Early September 1913). "As to staying here, well the very idea has not occurred to me. Not bad to be here for a year, but no longer, no thank you."

"September 8. I take a very active part in political events in this country, my second homeland. Yesterday I voted in support of a resolution censuring the British Government ... An official meeting of the Independent Labour Party adopted the resolution I have just mentioned by a show of hand. I voted for it, too. The Irish police had manhandled some workers in Dublin; so they called the police 'Russian hooligans'. I was quite glad to hear my native land mentioned..."

"September 20 ... On Sunday I attended a meeting of the 'Men's Union for Women's Suffrage'..."

"October 23. They keep away from 'coloured' students, shun them. They think of those as an immoral bunch. But I am sure that these are just caste prejudices. I, for one, will ostentatiously make their acquaintance whenever an opportunity arises."

"October 12... We have already heard two lectures. (...) I was struck by the audience. Apart from the coloured all are stupid, as if specially selected for it. The reason is clear — the whole lot are aristocrats."

"September 11... I've joined a Socialist Students' Club (that's secret from my parents). Its members are fine, pleasant people, not like the usual run of British students who go about with a strange mixture of arrogance and primness and sports as their only ideal. I spend much time with Dmitriev... He is a well educated man who is about to graduate from the Medical Department and go back to Russia to agitate among the peasants. As to his medicine, he'll use it in exile, he says."

"November 21 ... I began to read Marx's *Capital*. Unfortunately, I could not get it in German."

"January 18, 1914... I finally braced myself for some economic studies. It was London that did it. I visited the Russian library there and spent a lot of time in it catching up on illegal publications...

"Only a socialist can be a consistent anti-militarist."

"February 22. An Armenian, an acquaintance of mine, took me to the International Club. Its members are of 26 nationalities ... A Black from Trinidad described the plight of the Blacks in America to us. I told him about the Jews in Russia. I walked home with a Dutchman who had a three-barrel name and a Black from East Africa. By chance I said I was an opponent of official Christianity, and actually of all religion. I saw my Dutchman turn red in the face. It appeared he was going to become a missionary, the third I had met that evening ... Of late I've heard disgusting things about the British in their colonies but no first-hand accounts had come my way. My Black was brimming with rage. Breathless with anger, he told me about the killings of coloured people; he threatened that the coloured world would awake and sweep its oppressors off the face of the earth. He said that every Black person left Britain with hatred in his heart and dreamed of avenging himself ... I met such a great variety of men during one evening... This opportunity is one of the few good things about this hateful voluntary exile."

An entry in an exercise book: "(Edinburgh, January, 1914) ... Getting to know the sense of life is the goal of the present

generations. Many thinkers, artists, scientists, writers and others are endeavouring to get as close as possible to that goal. The greater the knowledge, the easier it is to grasp the sense of life. So no science, not even those often called useless, is a futile occupation. Getting at the sense of life is the *most* crucial problem. And when we achieve that goal, or even come closer to it, is a life and death issue both spiritually and materially for many, many generations."

Then World War I broke out. Continuing to study abroad was out of the question. At last the choice was made, and on his own — the Physics Department of Moscow University. The books on physics he had read that summer fascinated him. He had had very little idea before of modern physics. Now a crack had opened which he found enticing. He opted for Moscow University since friends of his from Elizavetgrad were there: Natalia Shuiskaya, Boris Zavadovsky and Boris Gessen. He left for Moscow and was enrolled as a second year student, the year spent in Edinburgh was credited in full.

* * *

Notes made in Moscow in 1915. "... Will I be able to give up the still unsubstantial 'politics'? I tried to quieten my inner voice in fear of the calamities that await those who go in for 'politics'. That was it: a future dedicated to science alone. And I failed to see that this would doom the soul to death. Or, in the long run, it would end in intellectual philistinism, which is probably the only fear that is deep-rooted in me. It is too easy to reconcile oneself to putting off 'politics' for a year. Getting involved in 'politics' was, it seemed to me, if rather vaguely, a trial of strength. I was quite disappointed to find out that the trial was such that I had to burn my boats. In general, my deterioration skipped my attention completely..."

The note is a significant piece of evidence. While still at Elizavetgrad, Igor Tamm joined the Russian Social Democratic Workers' Party (RSDRP) and soon became a member of the Menshevik-Internationalist faction. When I asked him why, when he joined the RSDRP, he turned Menshevik and not Bolshevik, Tamm gave a very simple answer: he could not see the finer points of the party programmes then, and besides, there were no Bolsheviks among his acquaintances.

In 1914 and 1915 the war was still supported by some sections of the society (even by some antigovernmental quarters) as a necessary defence of one's home. Such people were called **oborontsy** (advocates of defence). Yet certain revolutionary

circles were fully aware that this war was alien to the interests of workers and peasants, and that the defeat would only promote a revolution. These people were called **porazhentsy** (advocates of defeat). They hoped that the war would evoke a revolutionary situation in each of the belligerent countries. Tamm shared this view. From the autumn of 1914, Tamm, a confirmed internationalist, was convinced that the war would inevitably lead to revolution. He believed that revolutions would flare up in Russia, Britain, Germany and the Austrian-Hungarian Empire... He contemplated joining the army to carry on political agitation among the soldiers. The heavy losses suffered by the army prompted other intentions: he would do what he could to save the lives of the wounded, the innocent victims of an unjust war fought for objectives quite alien to the interests of the common people.

In the spring of 1915 Tamm joined the Russian Red Cross Society. Many of his university colleagues did the same.

Here is one of the entries in his notebook for 1915: "May 5. Reutovo. An artillery duel. An abortive attempt to accompany some wounded to the hospital. May 7. A crazy night. 300 wounded. May 10. In the trenches... May 24. Came under shrapnel fire for the first time. May 27. The second shelling (percussion shells). May 27 and 28. Instructed to find out about washing facilities and roads. In the trenches..."

* * *

In 1916 they began to draft students into the army. The prospect of going to the front as a combatant rather than a medical orderly and an agitator seemed a very stupid thing to do to Tamm, who did not share the defence-of-the-homeland pseudo-patriotic feelings. There was only one way out — to go voluntarily to some officers' training school, thus gaining as much time as possible. The conviction that the revolution was around the corner never left him. Along with several other students he made up his mind to join an artillery officers' training school. The volunteers had to pass a medical examination board, and Tamm was pronounced unfit for military service. For fear of being drafted, Tamm showed up at another medical board. A second rejection followed with a strict warning not to make another attempt. It transpired that the first board had found him unfit for all military duties. With his mind at ease, he could now return to his university. But could a student with Marxist-revolutionary ideas really be at ease in 1916, when he was convinced that the revolution was forthcoming?

* * *

Here is another letter[18] Tamm received in 1953:

"Dear Igor Evgenievich!

I know and remember you quite well. Like the first swallow, you came to Elizavetgrad in the first days of the February Revolution of 1917... Selflessly and with great inspiration you addressed spontaneous street meetings, including many held near the Elworth factory. I remember you denouncing the Provisional Government of Kerensky and those cowards the Mensheviks![19] Young and enthusiastic, you would sometimes climb trees and use them as rostrums for your fiery speeches! Known as 'Igor', you were quite popular with your audiences, and where you appeared they would listen to you with great attention. At that time I lived in Elizavetgrad and worked at the oil products warehouse there... I knew your father quite well, too — we both served the Elizavetgrad Municipality..."

In April, 1917 Tamm was elected to the Bureau of the Executive Committee of the Elizavetgrad Soviet of the Workers', Peasants' and Soldiers' Deputies.[20] In May 1917 he was sent to Petrograd as a delegate (with voice and vote) from Elizavetgrad to the First All-Russia Congress of Workers' and Soldiers' Deputies that met in Petrograd from the 3rd to the 24th of June (June 16 — July 7 new style), 1917. With the other members of the Menshevik-Internationalists faction, Tamm voted against "launching a sustained offensive all along the front-line" and against continuation of the war. From Petrograd he returned home to Elizavetgrad only to leave for Moscow in September to complete his higher education.

Tamm gave a brief account of the autumn of 1917 in a letter written late in 1957 to the writer K. G. Paustovsky. "I read your *Taras Shevchenko* in a tent on the Sagran Glacier in the Pamirs... Our lives, it appears, ran surprisingly parallel courses at the beginning of the new age. In the days of the October Revolution I lived in Moscow some 50 metres from your home in 22, Nikitskaya Street... On the first day of the Revolution I set out for the Soviet but a White Guard patrol detained me. They removed my revolver and escorted me back home (was it because of my student's uniform?). Korovin's house was on fire and it lit up our

[18] Sent to him by L. S. Stern from Kherson on December 19, 1953.

[19] Tamm was a Menshevik-Internationalist and denounced the Oborontsy Mensheviks who were for continuing the war. That stand often placed him abreast with the Bolsheviks.

[20] "Employment and Service Record". All the dates and entries from 1919 to 1920 are quoted from Tamm's "Employment and Service Record" which was compiled on December 2, 1932 by the Physics Research Institute of Moscow State University (42 entries on five sheets). Various certificates and other documents confirming the entries are appended.

room on the sixth floor..." On the night of November 3 (the 16th new style) the White Guards were smashed in Moscow, and Soviet government was established in the city. Staying was put out of the question, he just had to get back home to Elizavetgrad. Life was changing around.

Tamm went to the Ukraine to join his parents. In January 1918 he was made "Assistant Chief of the Elizavetgrad Labour Exchange and Instructor of the Elizavetgrad Trade Union Organisation".

When White Guards took the town, Tamm had to leave it. A few brief entries in his "Employment and Service Record" make interesting reading: "1918, May. Appointed Instructor of the Central Trade Union Bureau of Kiev".

"July. Arrests (four days running) by the German occupation authorities on charges of organising a general strike." "August. Departure to Moscow to complete higher education."

Some passages from Tamm's letter to his mother in Elizavetgrad: "I am up to my eyes in work... My Professor has said definitely that as soon as I am through my state examinations... he will recommend me for an appointment to the Physics Chair of the University. He mentioned in passing that I will have to go back to Britain for a refresher course. But so far these are just words... I stand a good chance of staying with the Chair, but that, too, so far is no more than a chance... My examinations have kept me very busy; the composition I had to submit was the greatest bother...

"I have had not time to look for a vacancy which means I have to live on borrowed money. My future salary as a Physics Chair staff member will be 500 roubles a month, but that is still a long way off. I am still living in a world of books, I never go out anywhere, and life seems to be passing me by. I want to get finally settled by Christmas, so that I can get on with my physics *in peace* as my own boss. I shall come for a while over Christmas, even if it takes crossing lines — I miss you all ... October 15 (new style)."

"October 30, 1918. Assigned to the Physics Chair to be trained for a professorial appointment at Moscow State University."

"Certificate issued by the Physics and Mathematics Examination Committee on October 31, 1918, No. 122.

"This is to certify that I. E. Tamm covered the curriculum of Moscow State University, passed the final examinations and has in virtue thereof been granted a First Degree Graduation Diploma."

Yet another excerpt from the letter Tamm wrote to Paustovsky:

"...Like you, I tried in the winter of 1918 to sneak from Moscow

to the Ukraine without a permit. But unlike you, I from Zernov by-passed Khutor Mikhailovsky and travelled in a horse-cart all the way to the Krolevets railway station..."

Entry 9 in the "Employment and Service Record": "February 1919. I have been appointed Head of the Adult Education Division of the Elizavetgrad People's Education Department."

Then another, very brief visit to Moscow. The First All-Russia Adult Education Congress opened in Moscow's House of Unions on May 6, 1919 and Tamm was a delegate.[21]

Tamm heard V. I. Lenin's speeches at the Congress (including his famous "Speech on the Deceit of the People by Freedom and Equality Slogans"), and Lunacharsky also spoke.

On July 31, 1968 I made a recording of his account of the event. "Lenin was not there either on the platform or in the hall. Somebody on the rostrum was tediously and protractedly holding forth about some trifling matter. Suddenly (I had a good view from my seat at the side) a small door opened behind the stage, and Lenin entered the hall. He moved stooped so as not to attract attention sitting down on the raised footing of a column. Hardly anybody in the hall could see him. He was only plainly visible to those like me, who had seats along the side wall. But on the platform they had clearly espied Lenin and gestured him to join them. Lenin only stooped still lower and waved a declining hand. The platform party became more insistent, pointing to a centre seat that had been vacated for him. But Lenin once again turned the offer down. He stopped looking up altogether and concentrated on the notes on his lap. All those who could see Lenin now ignored the speaker completely and stared at Lenin. The speaker was well over the time limit and the platform was uneasy, but Lenin just sat behind the column, his notes spread out on his lap, waiting patiently his turn. As soon as the speaker stepped down, Lenin made a few brisk steps to take the floor. There were shouts of welcome but Lenin raised his hand, and there was instant hush..."

Entry 10 in the "Employment and Service Record": "August, 1919. I went to Kiev to avoid the advancing White Guards." And the last passage from the letter to Paustovsky: "... In the autumn of 1919 like you I lived in Kiev (in the Shulyavka district) when Kiev was in the hands of Soviets. Upon mobilisation being decla-

[21] The majority of the population in pre-Revolutionary Russia were illiterate. After the Revolution of 1917, the education of the adult population was an important task set by the government — *Ed.*

red, just like you I made my way out of Kiev and reached Odessa;[22] and then the Crimea with a suitcase and false papers certifying that I was going there to teach physics at the University of the Crimea. On my way there (and later on in 1920 when crossing the front line back north) I had so many incredible adventures that very few people believe that my stories are really true..."

Let me retell one such adventure here, probably the most dramatic of them all, the way I heard it from grandfather. In the summer of 1920 he decided to get out of the Crimea (then under General Wrangel's control) and return to his native Elizavetgrad. The town had already been liberated by Red Army units of the 14th and the 1st Mounted Armies. Grandfather deliberately took no identification papers. They would be no use either for justifying his escape from territory held by the White Guards or for coming over to that under Soviet control. He crossed over safely — the front was not an unbroken line as in positional warfare. Tamm and a chance fellow-traveller stopped for the night in a deserted farm-house. They were caught in it by a Red detachment. Neither had any papers. So, naturally enough, they were taken for White Guard spies and faced execution! But as good luck would have it, a drop-out student was in command of the detachment. He frowned morosely listening to grandfather's explanations of who he was and did not believe that his prisoner was a graduate of the Physics and Mathematics Department of Moscow University.

"So you are a mathematician, are you? A tall story, most likely! But we'll check it. Here: develop the function into the Tailor series. And find the expression for the remainder term, too! If you can do it, you are a free man! If not — it's the firing squad for both of you tomorrow."

They gave Tamm a pencil, a crumpled sheet of paper and a candle, and then an armful of newly-mown hay and locked them up under guard for the night.

"Soon my companion was calm enough to hit the hay... As for me, sleeping was far from my mind: a sentry kept watch over us behind the locked door, and the calculations had to be ready for the morning."

Tamm's nerves were badly strained — not only his own life was at stake, but that of "the innocent companion".

"I was so up tight that my calculations went all wrong.

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[22] After Soviet Army units had retreated North, General Bredov in command of Denikin's troops at Kiev ordered "a general mobilisation of all men below the age of forty".

I planned the expansion right, but made an error somewhere and got in a mess. I worked until dawn, but hard as I tried I could not find that damned mistake!"

In the morning Tamm failed to come up with the right answer; but the Commander saw for himself that the calculations had been done by a man who knew his mathematics. Tamm asked him to point out the error.

"It has been a long time now since I could expand a function myself... Must have forgotten everything — it has been three years since I walked out of the University. Yesterday, well, I wasn't serious about shooting you, you know..."

They let grandfather's companion free, but would not turn Tamm loose, though they did feed him. The White Guards launched an offensive, and along with the detachment — as a "prisoner of war" — Tamm wound up in Kharkov instead of Elizavetgrad. There one of the soldiers was ordered to take Tamm to Soviet Counter-Intelligence Headquarters.

"It will not take them long there to find out what you really are..."

"The two of us set out together in a city without any papers and not a living soul I knew. A man with a gun was marching at my side. Suddenly, in a newspaper pasted to a wall for everybody to read, I saw a name I knew — Gaisinsky. He had been the editor of *Golos Yuga* (Voice of the South), an Elizavetgrad local paper, and he was a good friend of my father's. I managed to memorise his address... My guard, it appeared, was from Melitopol and did not know Kharkov at all." So he suggested that grandfather himself should find the way to their destination.

"We had been told how to get to the Headquarters, I knew the lay of the land in the city more or less and having seen Gaisinsky's address in that newspaper, I made up my mind to make a detour and try to locate him at his place."

When they came to the right house, grandfather talked his guard into dropping in there "just for a moment". They rang the bell. The door was opened by Gaisinsky's wife, who also knew him well.

"I started to explain to her what had happened to me. But my guard interrupted my story, telling me:

"'Come on, your moment has long passed!'"

The man had no time to lose: as soon as his mission was over, he planned to head for the railway station and get to Melitopol before the White Guards took the town.

"But I did manage to let Gaisinsky's wife know that I was being taken to the Counter-Intelligence Headquarters in Kharkov..."

The Gaisinskys helped with grandfather's identification, and so did his brother Leonid, who was urgently summoned from Elizavetgrad to Kharkov for the purpose. Grandfather's "Odyssey" was over, and he was released from detention. By November, with a letter of recommendation from A. G. Gurvich, Tamm was in Odessa to see Mandelstam.

* * *

Tamm dealt directly with technology but for a fairly short stretch of time, in the early 1920s. After a year's work at Simferopol in his capacity as a senior member of the teaching staff at the Simferopol University Physics Chair (from November 1919 to August 1920), Tamm moved to Odessa. There he was employed on the City Department of People's Education. Then for several months he headed the Odessa Area Department of Vocational Training. In January 1921 he was appointed a junior lecturer at the Physics Chair of the Odessa Polytechnical Institute. But things were very hard in 1921 and 1922 — Russia was half-starving, and a teaching job could not provide one even with the barest necessities of life. A small team of members of the teaching staff at the Polytechnical Institute had to look for some "lucrative" extra work. The Odessa Radio and Telegraph Engineering Factory had something to offer to the physicists. The factory started production in 1919 and operated until 1924. Mandelstam helped Tamm get fixed up. It was he who recommended the young Tamm to the Physics Chair and was largely responsible for his appointment as a laboratory assistant at the factory. With Mandelstam and N. D. Papalexi as supervisors, the laboratory was conducting various technological tests. A team of enthusiasts from the students and staff manned the laboratory where they began to repair X-ray tubes. Before long the relations between the team and the factory management became rather strained. All the enthusiasts were sacked. In his letter home dated January 28, 1922[23] Tamm wrote: "...Things at the factory have almost completely come to the surface — using cuts in staff as a pretext, today they dismissed Shchegolev, Stokhorsky, Beck and me... and a number of other team-members. ... The repairs of X-ray tubes under the supervision of our professor are still negotiated. But I am sure it won't work. I have been with the factory for a full year, picked up many useful skills. When conditions improve, I think I can make a living as a radio-telegraph tech-

[23] Quoted from a rough copy of the letter.

nician. But I hate it at the factory — double-dealing and intrigues all round. The trouble probably started when we set up our repair team — it was looked upon as competing with the factory itself. Now we shall go the whole hog. The Public Health Administration plans to give us a well-appointed X-ray lab, with pumps and some 3 million roubles for the equipment we need. Our reserve stocks will last us some two or three months, and the Institute will lend us a helping hand. I drew my salary some days ago — 500 thousand roubles, and I get 800 grammes of bread daily (and there is no bread now under 30 thousand roubles) (...) I like the work very much, what we do builds up knowledge and skills, which will be rewarded very handsomely in the future. Farewell, hateful factory! Long live the new stage in my life!"

As a temporary make-shift arrangement the team got a small corner of a room at the Institute. Then things changed again. The factory received an urgent government military order, to cope with which the team's services were essential. On April 1 Tamm got his lab assistant's job back. The team developed R-5 radio valves and other more powerful types for the Black Sea Fleet. Mandelstam and Papalexi led the project. On October 13 Tamm was again dismissed as "redundant", though with a flattering reference: "... While employed at the factory I. E. Tamm has displayed profound knowledge of radiotelegraphy and theoretical physics and has undertaken and fulfilled a number of research projects. His last assignment involved cathode valves and the organisation of a vacuum shop." Tamm's "redundancy" was explained by Mandelstam's resignation and move to Moscow. On the same day — October 13, 1922 — Tamm "at his own request was relieved of his duties as a junior member of the Odessa Polytechnical Institute teaching staff." He bid farewell to his Odessa friends and went to visit his parents at Elizavetgrad before following Mandelstam to Moscow.

Some passages from Tamm's letters home between 1922 and 1925 might be of interest to the reader.

On Monday, November 6, 1922 he wrote: "... I have been in Moscow for two and a half days now. My impressions are so numerous that they would make up a whole book. Since that is impossible, and sending the letters must not be put off, I'll take up some casual points that are very much on my mind now. ... In Kharkov I got a fast train from Batumi. The locomotive broke down and for three hours we were kicking our heels in the open country: still we got to Moscow in less that two days. ... I spent the first night at Zavadovsky's place, but he had a visitor, the Rector of Turkestan University. So I moved to the 'Metropol'

and put up at the Smushkovs' there.[24] They too have three rooms, though on different floors... It was great to meet family. Now to the important things. So far I have had two great disappointments. First, Mandelstam is not here yet, and his appartment has not yet been furnished. Apparently no final official invitation has been extended to him as yet. They think at the office that he might come in the middle of November, so D. V. we can expect his arrival towards the end of the month. Secondly, both Borises[25] offered me work with the Scientific Association at Sverdlov University. A food ration, *a room*, a salary,— everything one needs plus research work of one's own choice and, most likely, not more than four hours of lecturing a week ... I'll look up Predvoditelev today to get in the know on the situation here, and then I'll start looking around ... Things may sort themselves out in a few weeks' time after Mandelstam arrives. But get in somewhere I certainly will, nice and cosy — so I am very glad to have moved to Moscow. No vegetating here — Moscow is really throbbing with intellectual life."

In November 1922 Tamm was appointed to teach physics at Sverdlov Communist University and in February 1923 he took up the same post at the Moscow Institute of Railway Engineering. From May Day 1923 on he also taught physics at the 2nd Moscow State University. In 1923 Tamm finished writing his first scientific paper *Electrodynamics of the Anisotropic Medium in the Special Theory of Relativity*, which he had begun to work on in Odessa in 1922. He was 28.

And here is what Tamm wrote to his parents in April 1925 (the rough copy of the letter has survived): "... Many good things happened to me in the meantime. And I shall start by puffing myself up and then take events in their right order. I am not sure whether I told you ... that there would be a vacant professorship at the 2nd State University... They promised very definitely to put me up for it, though I myself was not so sure about the whole business... Of late they have not raised the matter with me ... but I have quite a few things to console me. My first work on relativity done a long time ago was, at long last, sent over to Germany and handed to Einstein himself; he found it to be 'sehr hübsch'[26] and agreed to have it published in *Mathematische Annalen*.[27] ...

[24] His cousin Mariya Arkadievna and her husband Vadim Smushkov.

[25] B. M. Gessen and B. M. Zavadovsky.

[26] "Very valuable" in German. Einstein's reaction was passed on to Tamm by Ehrenfest.

[27] Elektrodynamik der anisotropen Medien in der speziellen Relativitätstheorie (In Gemeinschaft mit L. I. Mandelstam).— *Math. Ann.*, 1925, 95, H.1, S. 154—160.

I am through with my work on magnetons, the paper has already been received by the editorial office of *Zeitschrift für Physik*.[28] I think that in some two or three months I will see it in print.

"This is how it all happened. I presented the paper at Lazarev's Institute. Academician Ioffe was over from Petrograd and happened to attend the meeting. He challenged my views and a sustained discussion between the two of us followed. I got very much depressed by the whole thing. Then it transpired that back in Petrograd when referring to my paper he used an absolutely different pitch. And their magneton physicist urgently asked me to send my paper over to him. Now after a meeting he is siding with me and has recanted his earlier view... Frenkel, who stayed in Moscow for some time, let me know that the Petrograd crowd — Ioffe too — regret that in the autumn they did not induce me to join their Institute. Frenkel is going abroad for 12 months on a Carnegie scholarship this year... He is sure that I will be the man to get it in 1926. The allocation of the scholarships depends very much upon Ehrenfest who visited Russia last autumn. And among... Moscow physicists Ehrenfest commended Frumkin and me. Frumkin is already being pushed for the scholarship... Will it work out that way? It is still very much open to doubt, but the very idea that it is at all possible is bolstering me up. There are two mathematicians at our Main Directorate of Electrical Engineering[29] ("Glavelectro"). I spoke to them about my research. As a result (though it was absolutely unsolicited on my part) I got a fortnight's leave today over and above my annual holidays! This is to give me time to write my paper and develop my method. Quite an unexpected boon. This is the best illustration of the broadness of the Soviet way of life..."

* * *

A brief digression is called for here. C. P. Snow, writing about science in general, noted that scientists who had begun research before 1933 remembered very well what the climate in science was like in those times. At the risk of evoking the displeasure of his readers he asserted that those who had not gone in for science before 1933 were not aware of the joys the life of a scientist had had to offer. The world of science in

[28] Zur Quantentheorie des Paramagnetismus.— *Z. Phys.*, 1925, 32, H. 18, S. 582—595, Taf.

[29] Tamm had a job with "Glavelektro" for several years (from 1922 to 1927) combining it with teaching physics and doing theoretical research work. His main responsibility there was theoretical development work.

the 1920s, in C. P. Snow's view, was as close to an ideal international community as one might envisage. He said one should not assume that the members of that scientific community were some sort of supermen, or were without the usual human weaknesses, but that in the 1920s there was an environment of good will and magnanimity is science, and those steeped in it became better people.[30] These observations introduce us to a wonderful world of personal relationships, and friendly and very fruitful direct contacts between all the top scientists. Thus there was an integral scientific community until the fascist elements prevailed in Germany, Italy and Japan, and the tragic World War II. Without this brief introduction (and I would not venture to try and do it better than C. P. Snow!) some points in the letters that follow might appear a little off-beat...

Way back in September 1924 P. Ehrenfest heard Tamm deliver his report on relativistic electrodynamics. The paper was clearly much to his liking for Ehrenfest was to propose that a Lorentz scholarship be given to Tamm. (The year 1925 saw the 50th anniversary of H. A. Lorentz receiving his doctorate and a Lorentz Fund was set up on private donations).

The "Employment and Service Record" has a January 2, 1928 Entry 33 which reads: "A trip to Holland and Germany at the expense of the Lorentz International Scientific Fund."

Tamm arrived in Leiden at the end of January. Here are some passages from letters he sent relatives:

"(March 4, 1928)... I goaded myself on for a week, but to no avail. Fed up, I dropped everything and went to the cinema, played chess, billiards and tennis, visited P. Zeeman in Amsterdam and attended a physics congress at Eindhoven. So I've had a bit of a rest and find myself in good shape again. In a few days time the four of us (the Shubnikovs, Tanya Ehrenfest and I) are to set off down south for a week on bicycles. I have been looking forward to it. But now I am not so sure I'm pleased at having my work interrupted. But I'll go anyway... Of course, it's not really the way to go about things: nothing that I have done is ready for publication. But new subjects and results are coming up at such a rate that I have no time for the tedious business of writing it all up for publishers. Strange as it may seem, I am still going up in Ehrenfest's eyes and he is putting in a good word for me everywhere... The Lorentz Fund... has in advance 'sold me' on the idea of staying some extra time at Leiden for 300 guldens (250 roubles), which

[30] C. P. Snow. *Public Affairs*. London., Macmillan, 1971.

suits me fine. The greatest thing about this trip abroad is that it has made me more certain about my ability, or, to be more precise, has generated faith in me.

"It is now definite that on April 23 Dirac will come here for three months. So I shall be able to learn something from that new physics' great genius. Though they say that Dirac is not a great one for words and you have to try very hard to start up a conversation with him. He seems to talk only with children, and they have to be under 10 ...

"Recently I have got to know the younger Ehrenfests much better. Pavel is a very nice, vivacious boy; Tanya... is shy and in many ways childish, though she is 22, but she is a nice girl... She is a very good mathematician... The physics congress at Eindhoven was lively. I met so many of the physicists during my trips in Holland that I felt thoroughly at home, just as if it were an assembly of Russian physicists ... I have no trouble in following papers in French, but I am absolutely hopeless when it comes to speaking...

"Today I received a reply from Bell (a fellow I know from Edinburgh). Our correspondence is developing into an uncommonly interesting and friendly exchange. Strangely enough, we have the same interests in science (though he is a chemical engineer), hold almost identical political views and both love climbing ... He suggests the two of us do some of the Alps at the beginning of August. I am not sure that I can afford the time and the money, but the offer is tempting..."

"(March 8, 1928)... The research Ehrenfest and I have been doing together has been pushed to the background. He wants to have it published at an early opportunity, but I am not ready with it. The day before yesterday I reported to Ehrenfest and Klein... on the extensive research I have done, and both of them fully agreed with it ... Klein asked my permission to describe it to Dirac whom he is going to see for a few days in Cambridge. He further suggested that *very important* conclusions could be drawn from it... And that is what I am doing now... Ehrenfest suggested that we should write to Einstein immediately, but I said we'd better wait until the inferences are confirmed by strict calculations... I am really in seventh heaven: yesterday I reported to Kramers, Ehrenfest, Fokker, Klein and Kronig on a work by Dirac. Ehrenfest was highly pleased with it. Today I visited Fokker at Haarlem; the day after tomorrow I shall see Kramers at Utrecht and some day this week Schrödinger is coming; and I have met Bohr. Ehrenfest wants to keep me in Leiden until June. At least, I have not disgraced myself."

"(April 26, 1928)... Ehrenfest and I have been working for a whole week on a problem which, it seems, defies solution. This has disappointed Ehrenfest more than me... He feels depressed all the time and complains that life has lost its joy for him. We sometimes walk together and have many heart to heart talks. Sometimes it seems to me that I might be his closest friend in Leiden. I feel so sorry for him. Just lately I have only met with failure in my research (and not only in my joint venture with Ehrenfest). Most likely I shall be invited to go to Groningen in the extreme north of Holland for a few days to visit the Institute of Physics there. Dirac is coming over here tomorrow evening."

(Rough copy — undated): "I feel like I have a permanent hangover. The last piece of research I was doing all this month has come a cropper... As to the first investigations I did here, well, vague doubts are creeping into my mind... Almost no positive results from my stay abroad. To be sure, I have learnt a great deal, but much more could have been achieved. I feel like an idiot. The cause of this depression is probably the ever growing criteria — Ehrenfest's standards are no longer high enough for me (though this does not imply that I feel in any way superior to him). The criteria to go by now are Dirac's. And compared to him I am just a babe in arms. Of course, it is still more stupid to measure oneself by a man of genius."

(Rough copy — not dated) "... With great patience Dirac is teaching me the way I should go about things; I am very proud that we have come to be friends..."

An excerpt from a rough copy of Tamm's letter to V. I. Yakovleva written on his return home from abroad: "Vlakhernskaya (Moscow Area), October 4, 1928 ... Early in spring my patron (Mandelstam) and we, the members of his staff, received an invitation to move to Leningrad to work under Ioffe. Ever since we have been in a state of complete suspense — one day we are going, the next we are not and so it has been going on for quite a while now. The issue has been settled and revised scores of times; it is most likely we will stay here in the long last. Mandelstam will be given a research Institute to lead, with laboratories three times larger, and funds; our theoretical 'line' will have two new readerships. If we stay here, we hope to launch a new research programme. While abroad, I lived for five months in Holland and for two in Germany. It is probably a law of nature that one's best hopes never materialise. Still I am quite pleased with my trip, especially with the first half of it, when my work was at its best. The last

two months were mostly spent at congresses or just sightseeing; there was some climbing in the Bavarian 'Alps', and I roamed in Harz, Nuremberg, Munich and Hamburg. But what pleased me most was coming together with Dirac. He and I kept company for three months and came to be very close. Dirac is a true man of genius. Do not smile that it sounds high-flown; I really mean it. I know that when I grow old I'll be telling my grandchildren with pride about that acquaintance of mine. And I hope to tell you all about it much sooner. I was late for our Russian congress... The highlight of the programme was a most important discovery that Landsberg and Mandelstam made simultaneously with Raman in India ..."

* * *

In 1930 Tamm scaled the Kargashili-tau Range in the upper reaches of the Tyutyurgy Glacier (14,000 ft) in the Caucasus. Apart from Tamm the party included N. N. Pariysky (team leader), the mathematician B. N. Delone, M. A. Leontovich and others. They were also joined by James Bell, Tamm's friend from Scotland. Grandfather often recalled with evident pleasure how he and Bell visited his aunt, M. K. Krauze, on their way back to Moscow. "All coated in dust, hungry and tired, craving for a wash and change of clothes, we appeared on my aunt's doorstep in Novorossiysk. We rang the bell. The maid opened the door and upon seeing two shady characters, unshaven and shabbily dressed, she slammed the door in our faces. The haversacks on our backs only added to her suspicions. We rang the door-bell a second time. The door opened a crack and we heard the maid ask: 'Now, who are you?' I began to explain to her that I was Mariya Krauze's nephew and had come to Novorossiysk specially to see her; and that my friend and I had no place but my aunt's to put up at — she was the only person I knew there... 'I shall not let you in. Mariya Karlovna is out, she is at her office.' Then the maid added with firm conviction: 'Mariya Karlovna couldn't have such a ragamuffin for a nephew!' And the door was slammed shut again, that time for good. The morning heat was hellish. We were completely broke. And a wolf could not have felt more hungry in winter! But Bell and I had to sit it out in front of the house until Aunt Mariya came home from work..."

On April 29, 1931 Tamm left Moscow on his second scientific trip abroad, this time to Cambridge in England and Rostock in Germany. His report on that mission has survived. Here are a few excerpts from it: "Berlin from May 1 to 2, Leiden (Holland) from May 3 to 7. I stayed longer at Leiden at the request of the Institute of Theoretical Physics to address a colloqium and talk with staff members ... I learned of the Leiden and Groningen research on the theory of metals (Professor R. de L. Kronig came to see me). I also familiarised myself with the experiments under way at the Institute of Experimental Physics and, among others, with those on superconductivity and the magnetic and thermal properties of metals at low temperatures."

"London — May 8; Cambridge from May 9 to July 3 with several breaks for visits to London and other places. I am now up to date with just about all theoretical physics research projects underway at Cambridge. My contacts with Dirac were of special value to me. Dirac is one of the leading physicists of our time, and my discussions with him on fundamental matters of quantum theory and the road ahead in it were very valuable. As a follow-up to his latest investigations I did a piece of research in Cambridge. Subject — Generalised Spherical Functions and so forth. I was attempting a mathematical investigation of the motion of the electron in an isolated magnetic pole field, the existence of which is ruled out in classical physics theory. However, of late the idea has gained new ground: Dirac's new research has proved the existence of such poles not to be at variance with quantum theory. Dirac thinks that such poles may be important to the structure of the nuclei of heavy atoms...

"I had extensive scientific discussions with Professor Kapitza, Drs Mott, Blackett and Gerney, and with the US physicists at Cambridge (Morse, Houston, Dennison and Dicke) ...

"Rostock from July 8 to 29. The most important part of it I see as my joint work with Professor Jordan on quantum theory. Unfortunately, the shortage of time allowed us to develop only the mathematical aspects. The most important and interesting aspects, the applications of the mathematical methods already developed to the physical problem at hand, had to be left for the future. What we are talking about is finding a real to supplement the formal synthesis of quantum theory and the theory of relativity. We have so far developed a non-associative algebra (or a 'softened-up' associative algebra). Interesting from a purely mathematical point of view, it may, as we see it, prove instrumental in investigating the above physical problem...

Everybody I met was most cooperative and obliging, and Dirac and Jordan took great care of me.

"The general attitude... to the USSR was very acute interest, and a number of people including Blackett and Virsma, showed an extremely sympathetic outlook on our country. Many of them have already been even several times in the USSR, while a few would like to come to work here on a permanent basis (Ehrenfest, Virsma and Gerney). As might be expected, I also came across a very negative attitude to the USSR, but, strange as it might seem, not among physicists.

"On the invitation of the British Society for Cultural Relations with the USSR I gave a talk at a meeting they held in London... I repeated it at Cambridge...

"It is not so much experiments completed that I am interested in as the work done in theoretical physics, treatment of new problems... and direct scientific exchange, dialogue and discussion... with a number of very prominent physicists. These results could not have been brought forth either by reading scientific journals or by personal correspondence with scientists abroad..." (October 21, 1931).

* * *

In 1934 Pavel Cherenkov, then one of Sergei Vavilov's post-graduates, discovered a new type of radiation that came to be known as Vavilov-Cherenkov radiation. Three years later (in 1937) Tamm and Ilya Frank provided a full explanation for the phenomenon. They proved the radiation was caused by fast electrons running through a substance at a constant velocity exceeding that of light in that medium. Ever since the theory of relativity had been formulated, it had been commonly believed that such radiation was impossible. Here is an excerpt from the text of the Nobel Prize lecture given by Tamm in Stockholm on December 11, 1958: "When our work was almost completed, Academician A. F. Ioffe drew our attention to a paper published by A. Sommerfeld in 1904 on the field of the electron moving at a constant superlight velocity. Sommerfeld calculated resistance to such a motion caused by electron-emitted radiation. The motion of the electron was only considered by Sommerfeld in vacuum. The following year the theory of relativity was born: the motion analysed by Sommerfeld was incompatible with it, and Sommerfeld's paper was completely forgotten.

For the first time in many years it was referred to in our paper of 1937."[31]

Frank and Tamm had sent a reprint of their paper[32] to Sommerfeld in Munich. Before long Tamm received a reply from Sommerfeld and several reprints of his own papers.[33] This is what Sommerfeld wrote (translated from the German — *Ed.*):

Prof. Arnold Sommerfeld
Munich, May 8, 1937
Dunanstrasse, 6

Most Esteemed Colleague,

Several weeks ago my attention was drawn to your fascinating work on radiation at "superlight speed". So I have availed myself of it through the library (the publications that I get as a Member of your Academy have of late been coming in with extensive omissions). That made it all the more pleasant for me to receive a separate reprint from you.

I have never thought that the calculations I did in 1903 would ever find physical applications. This case proves that the mathematical aspects of theory outlive changes in physical conceptions. It is quite clear that the calculations I did do not conform directly with Cherenkov's observations. The expansion into the Fourier series that you applied is more fitting for the case. On the other hand, the phenomenon, as presented by me, seems to reveal itself in a simpler form.

I am sending this letter and several reprints to you with a Viennese friend of mine in order to avoid all the problems involved in the "Grenzübergang."[34] I regret those problems very much and hope to live long enough to see the day when political relations in Europe become normal.

With sincere gratitude for your kind message,

Yours, A. Sommerfeld

Nothing genuine could leave Tamm apathetic or indifferent. Once, when the newest areas in biology came under discussion, he said: "I am a dilettante, and the weakness of being such in science is having a negligible amount of knowledge. But sometimes it can be an advantage too. One's vision can be broader — disregarding the particular one can take in what is going on in the field of science adjacent to ours."[35]

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[31] *The Nobel Lectures.* M., Fizmat Publishers, 1960.

[32] I. E. Tamm, I. M. Frank. "Coherent Radiation of a Fast Electron in a Medium". Compt. rend. *(Doklady) Acad. Sci. USSR,* Moscow, 1937, 14, No. 3.

[33] Sommerfeld A.— *Götting. Nachr.*, 1904, 99, S.363; 1905, S.201; Klein F., Sommerfeld A. *Theorie des Kreisels.* Leipzig, 1910, Bd.4.

[34] Play on words. Grenzübergang means crossing of the border and transition to the limiting case (a term used in physics) — *Ed.*

[35] Quoted from the shorthand record of his speech at a colloquium at the Institute of Biophysics, the USSR Academy of Sciences, on October 30, 1956.

This "dilettante" view often stirred considerable interest among the experts. In 1956 and 1957 Tamm delivered several reports and lectures on "Physics and Genetics". Here is an extract from a letter to Tamm by the noted cytophysiologist D. N. Nasonov: "...We have heard of your presenting a most interesting paper... Its subject is very much in our line. I would be most obliged if you could send over a copy of the text to me... If you have any plans to visit Leningrad we would be only too happy to listen to you at our laboratory..." (March 1, 1956). This is Tamm's reply to this letter:

"I am very glad that the interest in my paper is so surprisingly great... I look upon it only as getting thing rolling — now it's up to the experts ... There is something more to me than a mere dilettante, but I do not want to switch over to another science. I have simply read two articles by Crick (October 1954) and by Gamov (October 1955). I found them of such absorbing interest that I began reading into the subject and, in addition to talking things over with biochemists, covered quite a few scientific articles and no fewer pages in textbooks and monographs. This is the impression I have... the first results in genetics, often arrived at by quite roundabout ways, begin to be seen as directly related to quite definite chemical structures and processes..."

Now, a few excerpts from a lecture delivered by Tamm at Leningrad University: "...Ours is the age of biology. In the second half of our century it is to be in every respect a leading science. Biology will make substantive progress towards understanding the essential laws of nature. It will have the same kind of influence on lifestyles and the development of new branches of technology as physics has had... Biology is on the verge of great discoveries — a descriptive science is becoming an exact science. Darwin's theory is the greatest achievement of the human intellect. Some of our scientists underrate the importance of biology, and still its coming to a new level in revealing the basic macroscopic laws is proper. Physics must be linked to biology not only because the former provides new means of observation (the tagged atoms, X-rays and others), but because of late physics has focused its main efforts on bringing out the essential, elementary laws governing the development of a wide and varied system of phenomena..."[36]

[36] Quoted from the shorthand record of a lecture delivered by Tamm at Leningrad University (October 23, 1957, pp. 1—2).

* * *

An amusing story going back to two 1958 Geneva conferences[37] Tamm told us like this. The song *Do the Russians Want a War?* (words by Yevtushenko) had just become very popular. Grandfather liked it very much, and he jotted down the lines and learnt them by heart. One day in the conference lobby, he quoted the verse to an American physicist as proof of the strength of anti-war feelings in our country. Tamm translated the full text of the song into English and was very surprised by the reaction of his US colleague: the man's face darkened and he soon broke off the conversation. A few days later, still perplexed, Tamm decided to ask the American what had gone wrong. It transpired that the American colleague laboured under the delusion that the answer to the lines "Do the Russians want a war?" was in the affirmative: yes, they do. Tamm had to explain the exact meaning of the verse to him and the physicist brightened up at once.

In September 1965 Tamm was invited to attend an elementary particle conference in Japan. It was called on the occasion of the 30th anniversary of Hideki Yukawa's prediction of the meson. Tamm said modestly that a personal invitation from Yukawa was a great honour to him, for he held Yukawa in very high esteem... He was also very glad to have the opportunity to meet once again his colleagues Möller,[38] Rosenfeld,[39] Marshak[40] and Sakata.[41] Tamm was fond of all of them. Grandfather was very pleased to visit his native Vladivostok, a city which he had seen only once after a lapse of almost 60 years on his way to Kamchatka and the Kuril Islands in 1956. Grandfather was very eager to visit Japan. He only had very vague memories of it from his early childhood (when the Tamms had moved from Vladivostok to Elizavetgrad they stayed for over a month in Japan).

Tamm came back from Japan in excellent spirits, touched

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[37] The first on peaceful uses of atomic energy; the second on matters of high-energy physics.

[38] Christian Möller, a Danish physicist whom Tamm met first in 1928.

[39] Leon Rosenfeld (one of Bohr's team) and Tamm had immediately taken a liking to each other when they met in 1928.

[40] Robert Marshak, an American physicist, and Tamm were on friendly terms after they made each other's acquaintance in 1956.

[41] The Japanese physicist Shoichi Sakata and Tamm, after meeting in 1956, began to correspond.

by the "out of all proportion"[42] token of respect heaped upon him by his Japanese colleagues. For instance, "I was invited to chair the morning meeting at which Yukawa himself delivered his report... The conference over, they arranged for me a number of excursions around the country, explaining everything we saw and, in general, took a lot of trouble with me, showing me attentions I did not deserve!"

Looking at my sunburnt and buoyant grandfather nobody would have suspected that not everything had gone so well. It transpired that he had broken his arm. On their arrival at Kyoto, the guests were offered a choice between a traditional Japanese inn and a "European" hotel.

"Of course I plumped to put up at a Japanese inn," grandfather told us. "What was the sense of visiting Japan and living as if you were in America? Everything was extremely interesting! Folding screens, three wonderful flowers in a vase, the vase on the floor, mats, and no furniture, only huge wardrobes built into the walls. In the building, in the yard, in the parks — everywhere dwarf pinetrees in tubs. Most unusual, for a foreigner, to be sure... Soon, though, I began to regret my choice: no sooner did you appear in the hall than all the staff knelt down before you to show that they were awaiting the guest's pleasure... Certainly, their style of service was etiquette, not degrading servility, but such Oriental ways of being helpful were depressing. To ask for a European hotel now seemed rather improper. So I had to dash to the exit as fast as I could. Once I went down the corridor into the lobby, everybody knelt, I hurried up out into the street, my foot slipped and the next moment I was flat on the floor, like the Japanese around me. But I was not so good at tumbling down as they were! And here you are. The floor was extremely slippery. But that's all right. I am very much pleased, the country is most unusual. Let's have a look at sights of the ancient capital of Kyoto..." and he opened a huge album gifted to him.

As to the Conference, grandfather remarked that while only 38 all in all had been there, 15 of them foreigners, the concentration of "crazy" papers was very high, "but, alas, without that 'touch of craziness' that Bohr demanded — they were crazy

[42] Such special treatment on the part of Japanese physicists and Yukawa himself is explained by the fact that the starting-point of Yukawa's meson theory was Tamm's work "Neutron-Proton Exchange Forces and Fermi's Theory". *Collected Scientific Works* by I. E. Tamm, M., Nauka Publishers, 1975, Vol. 1, pp. 287—288 (in Russian).

in the common sense of the word. So my paper was in no way outstanding against the general background..."[43]

The report by Shoichi Sakata "A New Concept of Elementary Particles"[44] was, in Tamm's view, yet another "crazy" paper read at the Conference. Sakata introduced the notion of a "paint-like substance" and named it "B-Matter" without giving any explanations as to what exactly he meant by it. The paper postulated the existence of a "triplet of leptons; when a B-particle is added to it, it turns into a triplet of hadrons and begins to interact actively..."[45] As the word "paint" has several meanings in English, there was a lot of banter in the lobby. B-matter was jokingly called Beer-matter and the idea postulated in the paper was rephrased in the following way: "Three taciturn men (three leptons) are sitting silently. Let us put some beer into them and they will turn into three jolly fellows (three hadrons) ready to paint the town red (strong interaction)."

That flight to Japan was to be the last journey in Tamm's life. His health began to falter and then took a sharp turn for the worse. Getting tired easily, a state new to him, depressed him greatly. For a long time he had his own way of handling the problem: he dismissed it as non-existent. At first this worked.

A Solvay Congress was to meet in Brussels in 1967. "The Solvay International Fund Institute of Physics and Chemistry"[46] comprises two standing committees on administrative and science matters. In the 1960s Tamm was a member of the Science Committee which decided on the agenda of the congresses and on lists of scientists to be invited. The Statute of the Institute said that to qualify for an invitation a scientist had to be prominent in the area of science chosen for discussion or his work must be promising by way of contributions in this area. The agenda of the 1967 Solvay Congress was "Fundamentals

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[43] Igor Tamm was fond of quoting Bohr as saying that a genuinely new theory should sound crazy at first. In Kyoto Tamm read a paper "Curved Impulse Space". He ended it by saying: "Thank you very much for your attention to such problematic and crazy ideas." The paper is published in Vol. 2 of the *Collected Scientific Works* by I. E. Tamm. M., Nauka Publishers, 1975, pp. 218—225 (in Russian).

[44] Sakata, Maki, Ohnuki. Remarks on a New Concept of Elementary Particles and the Method of the Composite Model.— In: *Proceedings of the International Conference on Elementary Particles*, Kyoto, 1965. Kyoto, 1966, pp. 109—118.

[45] *Ibid.*, p. 110.

[46] This international body convenes congresses to discuss the key problems of modern science. Only a few scientists are invited to take part, so the selection is done with great care. Solvay Congresses are held once every three years.

of Elementary Particle Physics". Tamm was invited.[47] He felt optimistic and looked forward to going to Brussels with his daughter: "If the doctors do not let me go by myself, so much the better! We shall go there together, Irene..." In the autumn (they were to leave by the 1st of October) there was a sharp decline in Tamm's health, and he felt he could not go. He was sorry about it and would tell his daughter jokingly: "I've cheated you, and how! Failed to take you along to Europe and introduce you to Her Majesty the Queen of the Belgians...." When a photo album came from the Congress he (as always) was sincerely surprised to see an inscription on it in Gell-Mann's hand: "To our much respected colleague Igor Tamm with the best wishes and greetings." All the scientists at the 14th Solvay Congress had signed it. Among the names were Heisenberg, Möller, Weizsäcker, Gell-Mann, Marshak, Rosenfeld, Chew, Amaldi, Wigner, Sakata and Low. Looking through the photos, Tamm joked: "Well, there's no denying it — we would have been in excellent company." There were photos of everyone who had taken part in Solvay congresses from 1911 to 1967.

In 1968 Tamm wrote: "The absorbing interest evoked by physical problems defies any description. Apart from purely intellectual enjoyment, scientific research involves deep and varied emotions. You feel like a hunter chasing the truth, or a climber challenging high summits. New ideas and investigations make you feel like a true lover of music must, or so it seems to me, when listening to a great composer..." The reservation is fairly indicative, especially considering Tamm's strange relation with music which I have mentioned.

Tamm liked to repeat a Dutch dictum which he had seen in 1928 over the main entrance to the City Hall at the Hague: "Our past is in our present, and today we must create our future."

At the 6th Pugwash Conference in Moscow (1960) Tamm stated: "... We fail to understand each other because we very often give different meanings to the same words. But that does not mean that there is no difference of opinion among us. If I understood the report by Professor Kibal correctly, then I disagree with his proposals for a stable system of mutual deterrence...

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[47] The Science Committee of the Institute comprised in 1967 Amaldi, Bragg, Gorter, Heisenberg, Möller, Perrin, Tamm, Tomonaga and Geheny (Secretary).

"There are also matters on which we do not disagree in substance, it is just mutual understanding that is lacking... We should really take up concrete matters to see if there are any specific divergencies in our views... I mean if we really discuss concrete proposals, we might discover that on many issues we seem to have differences where there are really none..."[48] Tamm always stressed that the barriers of mistrust and misunderstanding must be cleared out of the way, and the sooner the better, that abstract speculations must give way to specific proposals, and effective actions must be the next step.[49]

And now, here is what Tamm wrote in 1961 during his stay in the United States: "Sept. 5th. Some night! Real dark... Smuggler's Notch, Vermont... There's a double suite opposite mine where John F. Kennedy stays when he comes here for a spot of skiing. The first day of the Conference wasn't bad at all, but I found it very tiring. I seem to be popular among the delegates... Although mine was a very brief speech, I then had to talk politics non-stop from 7 to 11 p.m., first with one group of Americans at dinner, then with another, and I beat a retreat from a third. It looks as if things will continue that way. It's very tiring indeed, but I feel I am doing a useful job..."

At the celebration party on Tamm's 70th birthday, this motto was presented, among others: "Academician Tamm begs scientists of the world to stop killings!" He took a terrific liking to this slogan, for a speedy stop to the arms race and lasting peace throughout the world were always on his mind.

In 1961, after the 7th and 8th Pugwash Conferences in Vermont, Tamm wrote a "Pugwash Anthem"[50] and read it out to his friends at the 9th Pugwash Conference at Cambridge in Britain in 1962. "We should work together to banish war from the face of the Earth," the words ran... The text has been preserved to this day.

These simple and ever so slightly naive lines catch the spirit of the Pugwash Conference which brought together scientists of goodwill from different countries. They conjure up in my mind a kitten snoozing blissfully under a blanket — an emblem on the calendar that American Cyrus Eaton, the founder of the

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[48] *The Verbatim Report of the 6th Pugwash Conference of Scientists*, pp. 34—36.

[49] J. Rotblat. *Pugwash: A History of the Conferences on Science and World Affairs*.— Czechosl. Acad. Sci., 1967, p. 42 (J. Rotblat — a British nuclear physicist, Secretary General of the Standing Committee of the Pugwash Movement).

[50] I. Tamm composed the text in Russian, and rendered it into English, right on the spot, at the Conference.

Movement, sent to every participant each year. On receiving this calendar, Tamm would hang it on the dining room wall under the clock. I can see Igor Evgenievich bursting into the room showing the pages of the new calendar... The memory is still fresh, although ten, twelve, fifteen years have passed.

S. V. Vonsovsky

AS FULL AS LIFE ITSELF

Mixing with people of great talent is a most soul-satisfying experience. And particularly if talent is combined with high moral standards. Such a feeling was common for all who had the fortune to meet Igor Evgenievich Tamm ... A most eminent and brilliant researcher in theoretical physics with the broadest of scientific interests, he was a kind-hearted man, always ready to afford assistance. High moral principles were integral to his character. Never did he act against the dictates of his conscience.

Igor Tamm is no longer with us and I deeply regret that we did not have the opportunity to meet more often. We lived and worked in different cities, and our contacts were confined to my occasional visits to Moscow; and I did not even see him every time I was in Moscow. Only once, shortly before his death, did I get to live side by side with him. That was for nearly a month at Kislovodsk (Caucasus) where we lodged in neighbouring rooms at the Gorky Health Resort and took our daily walks together. But more on that a bit later...

I first came into touch with Igor Tamm through reading his book. In 1930, as a third-year student at Leningrad University, I attended Professor V. Fredericks's lectures on electrodynamics. He recommended strongly a book by I. Tamm just off the press — *Fundametals of the Theory of Electricity.* The book was sold out in Leningrad but luckily my parents managed to get hold of a copy in Tashkent. It was a bulky book, bound in an attractive orange and yellow, and set in large beautiful type. We students derived real aesthetic pleasure from it. The coherent exposition of even the most involved questions displayed freshness of approach. One of my friends imagined the author to be a man of aristocratic stature just like his work. Those who knew Igor

Tamm's democratic make-up will understand how wrong this friend was.

I first met Tamm in the flesh two years later in 1932, at the Leningrad Physical-Technical Institute. An international conference on the theory of solids was held there. Semyon Shubin, who was to become my teacher and friend, had just been appointed head of the Theoretical Physics Department of the Urals Institute of Physics and Technology. I began my independent work there upon graduation from Leningrad University. A young but fairly well-known researcher, Shubin had studied under L. Mandelstam and I. Tamm. Shubin told us, callow youths as we still were, a good deal about Igor Evgenievich as a wonderful scholar and man.

The conference was also attended by such celebrities as Dirac, the Braggs (father and son), Fowler, Peierls and many others, so we got no time to talk with Tamm. Shubin just introduced us as his co-workers. But we were very proud of that and listened with fascination to Tamm's speeches, his questions to the speakers and his translations of papers from other languages.

Our next meeting took place later in the year, in December. The entire staff of our department — five people in all — went to Moscow "to warm ourselves." Our building in Sverdlovsk was not quite finished. The heating did not function and real ringing 40° C Ural frosts had set in. I remember how we one evening, led by Shubin, visited the red building in the courtyard of the old university on Marx Avenue that was the Institute of Physics of Moscow University. A seminar was to take place at Tamm's Theoretical Physics Chair. The seminars were usually held in Tamm's study, which was not a large room. Quite a crowd had assembled and all the seats were occupied by "people of consequence," while we huddled modestly against the wall. Igor Evgenievich had not yet arrived but many thronged the corridor.

Suddenly we heard Tamm's rapid speech. My wife, who had taken a course at Moscow University's Physics Faculty and heard Tamm's lectures, told me that his students had introduced a rate-of-speech unit — the "tamm"; the speech of an ordinary man or woman was expressed in "millitamms". When students asked him to slow down a bit, he smiled and said he hoped they were quick in the uptake.

There was some commotion at the door and Tamm's head emerged among the host of physicists but he was unable to force his way in. A blackboard standing at the other door had to be moved before he finally squeezed in. Clearly no seminar could

be held in such impossible conditions and we moved to another, larger room. I no longer remember the subject of the seminar — what I do remember is our rapture, how we with bated breath listened to Tamm "in person".

The seminar over, Shubin talked for quite some time with Tamm, and we stood nearby and listened to their dialogue with great interest. At that time, their scientific pursuits were very close. Their joint study on the theory of photoeffect in metals had just been published. In addition to that, they were planning a work on the quantum theory of metals, and they discussed it that memorable evening. That interesting project was to be undone by Shubin's premature death. And soon after Tamm's scientific interests changed abruptly to quantum electrodynamics and elementary particle theory.

Returning to the residence and cleaning ourselves of the chalk we had picked up from the walls of Tamm's study, we went over what we had heard there again. My personal, informal acquaintance with that wonderful man dated from that December evening in 1932. I shall retain my deep respect, affection and admiration for him to my dying day.

Shubin, from the very start of our work at the Theoretical Physics Department of the Urals Institute of Physics and Technology, had told us a lot about the purely scientific aspects of Tamm's work and his remarkable human qualities. A living Tamm would appear before us, with his ardent passion for science and uncompromising stand against any anti-science; his crystal-clear integrity; and his kindness and sympathy for all who deserved it. When someone came to him for assistance, he would not bother about the finer points but "take the bull by the horns"— "What can I do that will really help you?"

Our first few years in Sverdlovsk (1932—1936) saw Shubin maintain regular contacts with Tamm. They discussed in detail his work on the quantum theory of liquid metals. This work — now undeservedly forgotten — excited Tamm. Shubin would send his manuscript and received valuable advice. Shubin was working on what became known as the polar multielectron model of crystals (metals and semi-conductors) which synthesised, in a way, the Bloch-Peierls one-electron zone model and the Heisenberg multielectron homeopolar model. Shubin engaged me on that problem too, and I remember well the interesting discussions we had concerning the model he suggested. Today we make wide use of a simpler version of this model, usually named after the British physicist Hubbard.

In 1931, Dirac's magnetic monopole work came out, and Tamm showed a keen interest in it. He enthusiastically shared

with us his impressions of the possibilities offered by Dirac's new idea, and got involved in developing the theory. I remember how on one of our visits to Moscow Tamm told us of recent experimental work on observing fast particle trajectories in the Wilson chamber and that some of the photographs showed unusually thick tracks which had been dubbed "fat caterpillars". He suggested it might be the Dirac monopole itself, the ionization capacity of which, according to the theory, had to be quite considerable on account of the large magnitude of the monopole elementary magnetic charge. Tamm looked into the monopole-electron system and demonstrated that no stable states could exist in it.[1]

Shubin read for us young theoretical physicists at the Urals Institute a very interesting and useful course of lectures on the fundamentals of theoretical physics and on the then new areas of quantum mechanics and quantum statistics. He drew extensively on the recently published monographs of Weyl and Pauli (from the German "Enzyklopädie der Physik") and on von Neumann's formidable work. Tamm supervised our classes and gave us much valuable advice on choice of subjects.

Igor Tamm was an optimist by nature, full of *joie de vivre,* and he had a delightful sense of humour. He was a man who could take a joke.

I always made a point of seeing Tamm whenever I made a trip to Moscow. I called on him at his apartment. At first this was in the very heart of Moscow, then next to the old compound of Moscow University, and later in the neighbourhood of the Kursk Railway Terminal; and finally on Maxim Gorky Embankment. I tried not to miss his seminars when in Moscow. On several occasions, I was asked to speak at those seminars and even to present progress reports on my work in his presence at meetings of the Physics and Mathematics Department of the USSR Academy of Sciences. It was with great anxiety and trepidation that I waited for Tamm's comments. He was a straightforward man and always spoke his mind, no matter how unpleasant it might be for the person whose work he was assessing. But I was lucky — I do not think I ever came in for harsh criticism. Tamm's remarks were of great value: he went straight to the heart of the matter and helped you improve your work. I remember his favourable and useful comments on my paper in 1947 on application of the Landau theory of phase transitions to ferromagnetic-paramagnetic transition. Of no less significance to me

[1] See S. A. Altschuler's recollections in this book — *Ed.*

was Tamm's opinion on another report of mine at a session of the Physics and Mathematics Department of the Academy of Sciences in 1948, on the general multielectronic interpretation of solids. Tamm's well-intended criticism and profound understanding of the substance of the matter often opened the researcher's eyes to further implications of his work.

The reader might be interested at how Tamm himself took criticism. I recall us conversing once on Lev Landau. Tamm thought highly of that wonderful physicist and admired his works and keen intuition. I asked Tamm how he reacted to Landau's "tough" to the point of "scathing" criticisms. He was silent for a while, then smiled and said that he always found Landau's criticisms very useful. Landau's great critical mind never missed the weak points. The author was, of course, rudely brought down to earth, yet if he believed in his powers he would not lose heart and lament, but drink the bitter cup of criticism to the dregs as though it were some salubrious water and rush into battle toward ultimate victory. This attitude was an integral part of Tamm's outlook.

Tamm visited us once in Sverdlovsk. That was in May 1937, when a large group of Academy of Sciences members came to attend a special session held in this city by the Academy's Physics and Mathematics Department; the group was led by Academician A. F. Ioffe, the chief sponsor of the Urals Institute of Physics and Technology. Tamm made a detailed study of what our young researchers were doing, offering many valuable suggestions and comments to each of us. In those difficult times, Tamm remained true to himself, always prepared to help and give moral support to anyone going through a rough patch. Such things stick in one's mind.

Once the editors of one of our respectable physical journals offered a sharply-worded negative comment on a paper of mine. I felt the reviewers were hard on me and after some vacillation resolved to approach Tamm for his opinion. Were his opinion favourable, I would solicit his mediation in restoring the truth. He went through my article and found the editorial verdict too severe; his charming smile and kind eyes soothed me. He said there must have been some misunderstanding. With his assistance the article was accepted and saw print shortly afterwards. Tamm would not hear my thanks; he said the article had deserved to be printed and it was just a case of justice being done.

I remember yet another significant meeting with him. At the height of the Great Patriotic War against Nazi Germany, in November of 1942, with a colleague, Y. S. Shur, who is today a Corresponding Member of the USSR Academy of Sciences,

I was working at a munitions plant in the Urals. We had written our doctoral dissertations before the war. All major physical colleges and research centres, including the Physical Institute of the Academy of Sciences, the Physics Problems Institute and the Leningrad Institute of Physics and Technology had been evacuated to Kazan. Y. I. Frenkel, then my chief consultant on theoretical physics at our Institute, helped us organise the dissertation presentation ceremonies at the Leningrad Institute of Physics and Technology at the end of November. We were allowed to leave for Kazan for a few days. Y. I. Frenkel, E. I. Kondorsky and I. K. Kikoin were our official opponents. Unfortunately, I. K. Kikoin was summoned to Moscow urgently, and so our doctorates hung in the balance. What was to be done? We could not wait for his return as we had a deadline to be back at the plant.

Y. I. Frenkel took us to see Tamm. He said he would ask him to be our opponent — the dissertations were to be presented in just two days! That was our only chance. Tamm was very sympathetic, but said he could not very well take up Y. S. Shur's experimental work: "Your work, Yakov Savelyevich, is completely orthogonal to mine". However, he agreed to help me out.

We sat in the semi-darkness of his cold room, lit by the flickering wick of a solitary oil-lamp; I could see a heap of frozen potatoes in the corner. Nevertheless, Tamm worked with elan, his table was cluttered with closely written sheets, covered all over with most involved formulas. He talked in his usual debonair manner. To this day we cannot help looking back on it with a smile. I recall how he tried to "comfort" us and told, with his characteristically kind humour, of a physicist in Tbilisi defending his doctorate and not even getting a master's degree! Thus "comforted", we left Tamm and returned to the library where we slept on the tables. Tamm perused my thick doctorate and as always made many useful comments. Years later, on our Kislovodsk strolls, he would laugh heartily as I recalled that Kazan episode. For Y. S. Shur P. I. Lukirsky agreed to be his third opponent.

My joy knew no bounds when, arriving in Kislovodsk in 1967, I learned that Igor Evgenievich was there as well. However, he was in pretty poor shape and suffered from suffocating cough. Once a coughing fit occurred during one of our strolls and it was so bad that I got really scared. But he took his illness lightly, even with a touch of irony. The doctors insisted on his giving up smoking, but he was an incorrigible chain-smoker. It was only on our strolls, during our animated conversations, that he would not smoke. Even I tried to keep him

off smoking. He wrote his grandson in Moscow that he had a very strict nurse in Kislovodsk who kept an eye on him to stop him smoking but that he misbehaved now and then.

At the spa resort Tamm continued to work intensely. He was obsessed by a grand idea which he pursued in the last years of his life: to find fundamentally new ways of constructing microscopic theory that would make it possible to overcome all the drawbacks in existing quantum field theory. This objective, in his view, called for a drastic overhaul of the principles behind quantum theory. He persevered in exploring all possible avenues toward this gigantic development of theory. But death cut short that endeavour. His truly striking ideas were brilliantly presented in his report "Quantum Theory Evolution" read for him to the General Meeting of the USSR Academy of Sciences in 1968, when he himself was already seriously ill.

Even during our walks in Kislovodsk he just could not get away from his current topic of research. He always talked with great enthusiasm and although listeners could not always grasp all he said, one thing was perfectly clear — his whole life, all his aspirations centred on the search for truth. It was this that made being with him such a delight. He often abruptly cut himself short in the middle of a phrase, looked caringly into his companion's eyes and with the words "There now, I must have tired you out with my problems" turned to another subject. But Tamm was also an exceptionally good listener. He could tune himself in so well that his companion always felt perfectly at ease.

A typical sight in Tamm's spa resort room was his writing-desk scattered with sheets of paper containing most sophisticated calculations and, sad as it may be, with ashtrays full of cigarette-buts. Every walk with him was pure bliss for me. That happy month in Kislovodsk will stay with me for the rest of my life. I thank fate for having given me the chance to be so close to Tamm for such a long time.

When I went to see him, for the last time as it turned out, I found a courageous fighter, the same Igor Tamm with his fine sense of humour, his kind smile and his generous, open heart...

Many years have passed since Igor E. Tamm left this world. Yet all of us who knew his dear countenance, who heard Tamm's unique voice, who were warmed by his kind smile, feel as if he is close to us, living, for he was always as full as life itself.

E. K. Zavoisky

A MAN WHO WAS ALIEN TO INDIFFERENCE

I always dreamt of knowing Igor Tamm better but he was constantly surrounded by a dense crowd of theoreticians, so when I occasionally got to see him it tended to be in his leisure time.

I was at his place for the first time in the late 1930s to discuss setting up a Theoretical Physics Chair at Kazan University. He recommended to lead it a postgraduate of his, Semyon A. Altschuler, who had just completed his dissertation on the magnetic moment of the neutron.

Today we all know what a fine recommendation this was: Altschuler quickly achieved brilliant scientific results. In 1941 with the German invasion he volunteered for the front. From the end of the war he has headed one of the largest radio-spectroscopy schools in the world. Altschuler today, just like his teacher, has many followers but as distinct from him, owing to certain new laws in physics, he likes to experiment himself.

Tamm actively supported our endeavours and his criticism was always tactful and benevolent. I remember one case in particular. In Kazan where the major physics bodies, including the Lebedev Institute, had been evacuated, there was a meeting of Academy of Sciences members. The Rector of Kazan University took the floor and suggested that they should head the University chairs. In a way the idea might seem perfectly reasonable, but the form was humiliating to Kazan scientists for the Rector said: "In skill our scientists are comparable to the lab-assistants at the Academy."

Everyone was indignant and when I offered Tamm my Physics Chair he said he was puzzled by the Rector's statement and most certainly would not accept the offer.

At the end of the 1940s, on a joint academic journey Tamm and I once met late in the evening in the canteen after a long, tiring day. Our way back to our accommodation took us past a smelly little cellar joint that reeked wine. Tamm suggested a game of billiards. As we descended the battered staircase I tried to talk him out of it, for the nearer we got to the door, the more the joint stank and the louder the noise and swearwords. But nothing doing: he was determined to play. Words fail me to describe the place, but we picked our way through to a passable table and started a game. Tamm really played to win and his attitude towards his blunders was merciless:

"Idiotic!"

And to my soothing remark: "But you didn't give me an easy shot!" I heard: "Oh, come off it. It was simply lousy!"

I was a better player than he but I didn't know his one weak point yet: a defeat was instantly followed by a childlike pining, but this quickly gave way to a gay mood.

We later were to play tennis quite often. I had an obvious advantage in age but I used to get quite nervous when I played: I was afraid of making my blunders too noticeable and him seeing this as I wasn't bothering or, even worse, making concession to his years. When anything like that did happen he just said "Right then", as if passing sentence, and tactfully ended the game.

At chess Tamm almost always beat me. His manner of playing chess was easy and as zestful as in all other games.

Once there was a terribly boring and long doctoral dissertation defence and everyone was rooted because the contender was skilfully hypnotising us with graphs and charts. At last I. V. Kurchatov stood up and straightened out his broad shoulders. Everybody joyfully jumped up out of their chairs and gathered around him. At this moment I caught sight of Tamm, who with a cunning smile now went behind me from one side, then from the other: Professor A. was hiding behind me. Tamm suddenly caught him and with lightening speed pushed him under the table (for a bet). Then, as if nothing had happened, he joined Kurchatov and began talking with him about the need to help the biologists.

I always attended any meeting where Tamm was scheduled to speak. I heard the first report on the "Tokamak" thermonuclear system. Tamm took credit for a major part of this project. The cosmic boundlessness of the figures was stunning. Today physics is really getting to grips with "tokamaks" and the whole of humanity is eagerly watching what may well be man winning the energy to fuel tomorrow's world.

Our last and, alas, sad meeting took place not long before Tamm's death. A large noisy machine was dosing his breathing. I was struck by the steadfastness of his wife Natalia. She was staunch through her long, self-denying fight for the life of that unusually kind man as she had always been devoted.

Tamm greeted me energetically as ever, paid me some compliments and promptly switched the conversation over to the future of the sciences in our land. Though well up on world developments, he came back again and again to whether errors, similar to those in biology, could ever again be repeated in our country. This concern denied nearness of death or any letting up. It showed real conviction in the importance of the cause. He, like a dying soldier, was anxious to pass on the banner.

BRIEFLY ABOUT THE CONTRIBUTORS

S. A. Altschuler (1911—1983) — theoretical physicist, Corresponding Member of the USSR AS. He was a postgraduate under I. E. Tamm, studied and worked in Kazan together with E. K. Zavoisky. His major research (after the research into nuclear physics done jointly with Tamm) was in electronic and nuclear paramagnetic resonance.

E. L. Andronikashvili (b. 1910) — experimental physicist, Member of the Georgian Academy of Sciences, organiser and director of the major Physical Institute in Tbilisi. He is known for his experimental researches on liquid helium carried out at Kapitza's Institute in Moscow, as well as in Tbilisi, on physics of nuclear reactors and radiation material, on cosmic rays and biological thermodynamics.

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I. M. Frank (b. 1908) — experimental physicist and author of several theoretical papers. Member of the USSR AS and several foreign Academies. Up to 1970 he headed the atomic nucleus laboratory at the Lebedev Institute, and since 1957 has been directing the neutron physics laboratory at the Joint Institute of Nuclear Research in Dubna. His main works deal with optics, radiation of fast moving charged particles (together with I. E. Tamm explained the Vavilov-Cherenkov Effect and provided its theory; Nobel-Prize winner), nuclear physics, neutron physics and physics of reactors.

V. Y. Frenkel (b. 1930), physicist, professor (the Leningrad Ioffe Physical-Technical Institute). His main works of the last few decades are on the history of physics (books about P. Ehrenfest; about his father, noted physicist Y. I. Frenkel; about A. F. Ioffe and others).

V. L. Ginzburg (b. 1916) — theoretical physicist, Member of the USSR AS and many foreign Academies. After Tamm's death he took over the TPD, the member of which he has been since 1940. Professor (head of a chair) of the Moscow Physical-Technical Institute. His major works are on plasma physics, astrophysics (the origin of cosmic rays, radioastronomy), solids, optics, superconductivity, radiophysics, elementary particle physics, electromagnetic radiation of fast moving particles, as well as on philosophy and history of science.

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B. G. Kuznetsov (1903—1984) — professor, author of many books on the history of science (*Galileo, Einstein* and others) and the philosophy of science.

M. A. Markov (b. 1908) — theoretical physicist, Member of the USSR AS. Since 1934 he has been with the Lebedev Physical Institute (at the TPD up to 1949). At present, he heads a theoretical group at that Institute, and does research at the Joint Institute of Nuclear Research (Dubna) and the Institute of Nuclear Research of the USSR AS. His chief works are on fundamental problems of physics in general, quantum field theory, elementary particle theory and cosmology in particular. Author of papers and a book on philosophical problems of physics.

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